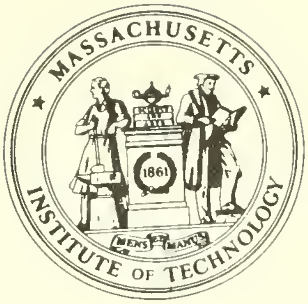

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ECONOMIC STRATEGY FOR DEVELOPING NUCLEAR BREEDER REACTORS

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Paul W. MacAvoy

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Preface

This study was fostered by particular conditions of both "demand" and "supply", and it may be appropriate to mention them at the outset. The author, while a member of the staff of the President's Council of Economic Advisors in 1965 and 1966, found quite evident demands in both government and industry for a broader evaluation of the breeder reactor development program than those coming out of the Atomic Energy Commission. These demands were quite evident because they existed for every program under the large umbrella of the Bureau of the Budget's PPBS (Planning-Programming-Budgeting-Systems); but in this case they seemed more extensive because of the large scale of the program and the "monopoly" on assessment established by the Atomic Energy Commission (however "natural" that monopoly might be). There seemed to be a need for "outside" evaluations and also for more perspective -- for consideration of national benefits from the breeder rather than only of promised technical advances in efficiency of uranium usage. The breeder reactor reviewed in the context of United States economic development might well justify a different total and composition of expenditures than those now envisioned.

There has been little or no demand for a study of whether there should be a breeder reactor development program. The decisions on such an undertaking have been made, and no amount of extremely critical review would lead to reversal of these decisions. Interest centers on alternative breeder programs -- of longer or shorter duration, of smaller or larger size -- offering greater gains. Thus this study takes the forecast rate of return on

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the present breeder reactor development program to be a good estimate of the alternative cost of a dollar spent on other reactor programs designed to gestate more or different types of breeders than that in the present plan.

The conditions of supply have been lavish. A grant from Resources For the Future has made possible the author's ventures into the Department of Nuclear Engineering at MIT and elsewhere, and research assistance whether by hand or on the computer. Nuclear engineers in all parts of the country have patiently responded to questions the likes of which they had never before encountered. Mr. John Bewick, sometime engineer at the Bettis Atomic Laboratories and now D.B.A. candidate at the Harvard Graduate School of Business Administration, provided critical first estimates of the costs of research in chapter 4 as well as an exceedingly constructive review of the first half of chapter 5. Mrs. Eva Ewing, of the graduate program in the Economics Department at MIT, contributed gracefully to the design of the measures for assessing buyers' gains in chapter 5 and wrote the computer programs for calculating these gains. More has yet to be supplied before this first draft becomes a publishable document; may the list of suppliers grow longer.

Paul MacAvoy
March, 1968

1. QUESTIONS IN REACTOR RESEARCH AND ECONOMIC APPROACHES TO ANSWERS

With the recent introduction of nuclear reactors into all of the larger electricity generating systems in this country, the Federal Government reactor research program has gained some breathing space. The basic research on the two successful "light water" reactor types has been completed in substance if not in form, with the collection of information on long term operating performance the only continuing requirement. Those programs not able to promise reactor types which can exceed the performance characteristics of the two commercial types have been shut down or at least reduced to a much lower level.

The breathing space, however, if not entirely past, will certainly be short. The Atomic Energy Commission's view that "to achieve widespread commercial use of nuclear energy for the production of heat and electricity . . . will require the development of fast breeder reactors"[1] has made an immediate need for new and comprehensive reactor research planning. The "breeder" projects will require scarcely less than $1 billion and perhaps more than $3 billion of long term commitments to new research now so as to attain "widespread commercial use" in the middle 1980's. The new projects are large and indivisible, and some promise the same results as others but from solving different research problems, while other projects promise quite different results in terms of operating performance and costs. Choices have arisen and have to be made presently to achieve any results

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[1] Milton Shaw (Director of Reactor Development, U.S.A.E.C.), "Fast Breeder Program in the United States," London Conference on Fast Breeder Reactors (17-19 May 1966: British Nuclear Energy Society).

for the 1980's and 1990's.

The purposes of another Federal research program in reactors are diverse and somewhat inconsistent. The breeder reactor is to be pursued to reduce the demands on the known stock of uranium reserves -- since it "breeds fuel" of its own from low-productive or partially depleted uranium -- but alternatively to increase the demands for the presently low-utilized plutonium byproduct from present commercial reactors. Increases in reactor efficiency are sought in and of themselves, where efficiency is measured both in terms of the ratio of fissionable material produced to that consumed, and the ratio of electrical capacity to fuel inventory.[2] But the overriding consideration is developing a power plant considered by the generating companies to be superior to fossil-fueled boilers or presently-available nuclear facilities: "the objective of the fast breeder effort is the development of a safe, reliable, and economic power plant for the utility environment."[3] The economics of buyers' demands indicates either favorable results -- reduced uranium fuel utilization which reduces costs and prices to the generating companies below those of alternative plants -- or that the projects, regardless of high technical performance, are without merit.

Matters of choice arise in allocation of expenditures among projects, and in project administration, as well as in the results. A number of paths can be taken in research to achieve a particular result. One of these might

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[2] These two ratios cannot both be increased at the same time, as will be seen below.

[3] Milton Shaw, op. cit., p. 3.

be chosen, or a number of parallel projects might all be undertaken; in fact, three of a large number of breeder projects have separate and important claims for exclusivity or for participation in joint programs. Each of the three has some basis for these claims in the economics of research cost savings: "the steam-cooled concept would make maximum use of water plant technology and the familiarity of the utilities with water-steam plants . . . the gas-cooled fast reactor concept would make use of technology which has been highly developed in the United Kingdom."[4] The Atomic Energy Commission's choice of the third as the dominant candidate for research has not been dislodged by economic comparisons with the other two: "It appears unlikely that the economic and technical comparisons will give any significant advantages to either the steam or gas cooled concept to the degree necessary to provide reasons for changing the high priority assigned to the sodium cooled fast breeder reactor in the U.S."[5]

The organization of projects into a research program is an economic matter as well. Two or three projects could be funded in order that the companies producing final demonstration reactors are separate and "competitive." Alternatively, the projects could be consolidated and contracts let so that each final producer specialized in certain components or certain plant sizes and no two companies offered new equipment with the same specifications. The decision between these possibilities -- and among those within the two extreme cases -- depends upon the relative costs and

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[4] *Ibid.*, p. 39.

[5] *Ibid.*, p. 39.

prices for the various qualities and magnitudes of plants built after the research is complete.

The policy planning questions in the new, large scale Federal breeder research program are both "how many projects" and "how many companies." The answers are acknowledged to be in economic terms; but what terms these actually are in the case of long term and uncertain research needs to be considered in detail.

The simplest economics of project selection is found in benefit-cost analysis. A quantitative evaluation of costs and benefits from a proposed research and development project is a fairly straightforward matter. The work sheets of an engineer concerned with "cost-effectiveness" will show dollars of revenue generated from each dollar of project costs. The PPBS (planning-programming-budgeting systems) review of a Government analyst shows expected project results which, when recast in engineering terms, can be used to find this same "cost-effectiveness." By both approaches, the costs are assigned to the project and year which seem appropriate and then are discounted to present values with an interest rate which seems appropriate. Benefits are the expenditures of final consumers on goods and services derived from the research, and any general environmental benefits, net of all costs of producing the goods and services and also the opportunity costs of obtaining the same results by the best alternative means. These benefits are discounted at the appropriate rate of interest, as well. Where the present value of benefits exceeds that of costs -- or where the benefits per dollar of costs exceed 1.0, so that costs are "effective" -- then the project is said to merit consideration.

Is this sufficient to justify actually undertaking particular research programs? To this point in time, the answer has been in the negative. A number of successful research projects can be pointed to which did not show cost-effectiveness before they were begun; and there is another number which had eminent cost-effectiveness but which were not undertaken. In the case of nuclear research, the submarine reactor must have been an example of the first (although not all of this project's *a priori* "benefits" can be known at this time), and power reactors fueled with natural uranium and moderated with heavy water -- as in the Canadian reactors -- must have been an example of the second in the early 1950's. Certainly enough examples can be found to make it difficult to explain past allocation of research funds on cost-effectiveness principles.

There are good reasons for carrying on research other than that shown by cost-effectiveness calculations. The high "cost-effectiveness" may not justify the risks that characterize this research. For one, the cost-effectiveness of a nuclear research project might be shown to be as high as that for investment in a new bridge; but the probability that the expected benefits and costs will be realized in the first case may be only 1/10 that in the second. The most likely reaction is to defer the nuclear work in favor of building the bridge, when there is competition for project funds, even though the expected returns are the same. After all, most Government officials and corporate officers may find it difficult to justify to the final claimant -- the voter or stockholder -- wagering all on the long shot project because it is the long shot.

Another good reason for going beyond the benefit-cost calculations is that, in some cases, no single estimate can be made of project results which carries more weight than other estimates. In other terms, estimates are made of the probabilities of each of a range of benefits net of costs. The research processes are evidently subject to some extent to probabilistic laws, so that there is only some empirical basis for predicting the average costs of research, or the average demand for its results. All that can be done is to describe the project in the researcher's terms, to show his expected results and perhaps subjectively state the likelihood of departures from these results. The framework for quantitative evaluation adds little towards deciding whether to undertake the project.

Can economic analysis assist in providing a framework beyond cost-effectiveness to guide decision-making on such uncertain research projects? The answer provided here is that the economic theory of investment can be of substantial assistance in dealing not only with problems of estimation in a benefit-cost study, but also can help to formulate a rational reaction pattern to the substantial risk which accompanies research and development.

Economic Evaluation of a Research Project Without Explicit Consideration of Risk

Consider the demands for the single product of a research project to be shown by the quantity $Q = f(P, Y)$ for prices P and incomes Y. The gross gains from successful research, given this demand, are equal to the total amount which consumers are willing to pay rather than continue in a world without the product. These benefits in one year are the sum of two

revenue streams shown in Figure 1: with the curve DD indicating the quantity $Q = f(P, Y)$, the first is the area P_oQ_o indicating total payments for the amount Q_o at the going price P_o, and the second is DDP_o indicating the amount the consumers gain on less than Q_o from having to pay only the marginal price for all intramarginal quantities. The second area, which is a graphical illustration of "consumer's surplus," is relevant in a research project but not a typical plant and equipment investment project because accepting or rejecting research implies either Q_o or zero output -- all or nothing -- while plant investment typically results in marginal additions to output.

The net gains to society from this research are equal to the present value of these annual gross gains, minus the total costs of the final product and, of course, the costs of the research itself. Production costs each year are $C = f(Q)$; they are shown as the area below the marginal cost curve $MC(= dC/dQ)$ up to Q_o for that output demanded at price P_o. The gains inclusive of research costs but exclusive of production costs then can be shown as the shaded area in Figure 1. The shaded areas for all relevant years, added together at present value (after discounting), can be termed total production gains. Net research gains are equal to this amount minus the present value of the costs of research; or, alternatively, the rate of return on research is that rate of discount which equates the dollar value of the shaded areas with the dollar costs of research.[6]

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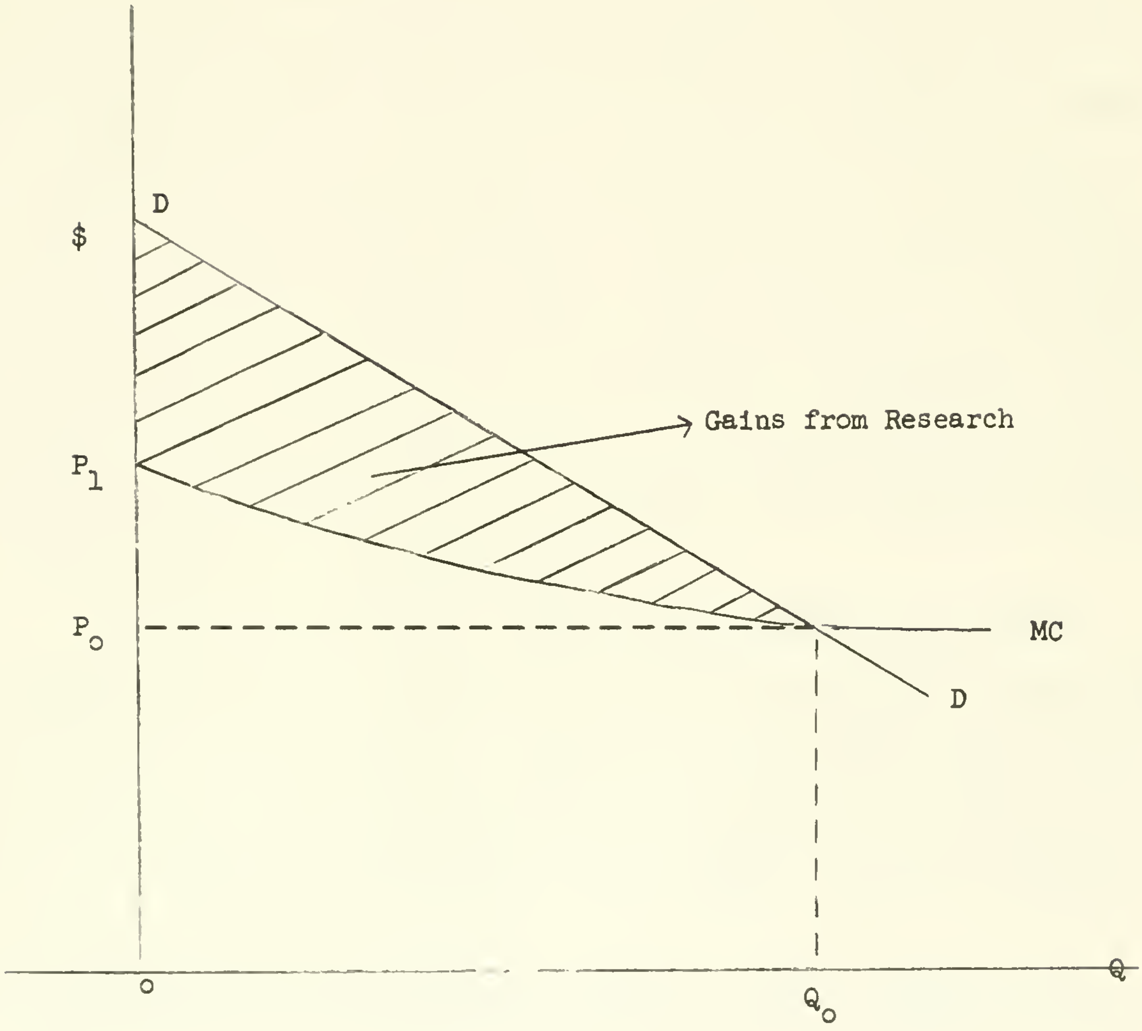
[6] That is, the rate of return such that $\sum_{t=n^*}^{n} G_t/(1+r)^t = \sum_{t=1}^{n^*} K_t/(1+r)^t$ for gains as defined in the text and research costs K during the initial periods 1 to n^* before completion of a demonstration reactor.

Figure 1

The Market for Research Output Q



These gains from the project can be made small or large by the output policies of the organizations carrying out successful research. For the largest possible gain, the market output should be Q_o because the value of the last unit as shown by its price is equal to the costs of resources used to produce it. But if the decision-making organization is a single private

corporation, then this amount Q_o may not be its most profitable output. All gains between P_1 and P_o accrue as surplus to final consumers; all the remaining gains between P_o and MC to the company. To maximize private profit returns, output Q should be less than Q_o because a smaller output adds to the difference between price and marginal costs [the most profitable company output is that for which profits $\{P \cdot Q - C(Q)\}$ are a maximum, rather than that for which buyers' gains $\{\int_0^Q PdQ - C\}$ are a maximum, and the first implies lower output].[7] Restriction of output in this way requires monopoly or oligopoly power -- power to change the total amount of the product offered for sale. The restriction, if practiced, costs the consumers more than the producer gains, by an amount shown by the price-marginal cost difference for all of the units by which output is reduced, so that there is a net social loss.[8]

The difference between benefits and costs shown in a cost-effectiveness study may approximate the returns calculated by these areas. But the calculation technique implicit in these analyses makes it likely that the estimated "cost-effectiveness" is less than the social gain per unit of expenditure from research, on two counts. The first disparity

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[7] The first-order conditions for max $\{P \cdot Q - C\}$ are $P + Q\frac{dP}{dQ} - \frac{dC}{dQ} = 0$ while those for max $\{\int_0^Q PdQ - C\}$ are $P - \frac{\partial C}{\partial Q} = 0$. The first implies a higher price and thus a lower output since $dP/dQ < 0$ (the lower the price the larger the quantity demanded).

[8] A single-buyer policy would have similar effects in the opposite direction: with the cost curve shown and the market rule that all units are sold at the same price, max $\{\int_0^Q PdQ - (dC/dQ)Q\}$ requires that $Q > Q_o$.

created in accounting is in the treatment of consumer's surplus. If the results of research include a new product, rather than only a cheaper way of producing an existing product, then the economic argument is that the value of research exceeds potential expenditures on the product by an amount shown as the shaded area under the demand curve. There is "consumer's surplus" for new products,[9] and since the research generates the entire market -- there is zero output if the project is not put into effect, and positive output from taking on the project -- it can claim all of the surplus. The amount claimed may be important: in Figure 1, this surplus is not only a source of gross benefits, but is the only source of net benefits.[10] The second disparity lies in accepting the price as the basis for finding cost-effectiveness. The economic argument goes beyond the firm's pricing policy for estimates of maximum net benefits alternative to those implied by that policy. Any price higher than P_o reduces social returns; but P_o is not the best price for an individual company with cost conditions as shown so that this price will not be charged if the firm can avoid it. Cost-effectiveness estimates based only on prices given by firms show less than full benefits.

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[9] As a matter of definition: "The product" in the relevant market includes all goods, of whatever physical description, which are close substitutes. If the product can be defined in an actual case so as to show the relation $Q = f(P, Y)$, it must be because all goods produced and sold in the relevant region are not substitutes.

[10] These cost and demand conditions may not be the "general case," so that pricing of research output may not be a "public policy" problem. There are other cost conditions with equal a priori plausibility -- the cost function $C = f(Q)$ can have positive first and second derivatives -- so that the general case is one that describes the greatest number of American industries. There is scant information as to which cost function does that. The illustration here is used to bring out important characteristics of benefits from nuclear research.

Economic Decision Rules Under Risk and Uncertainty

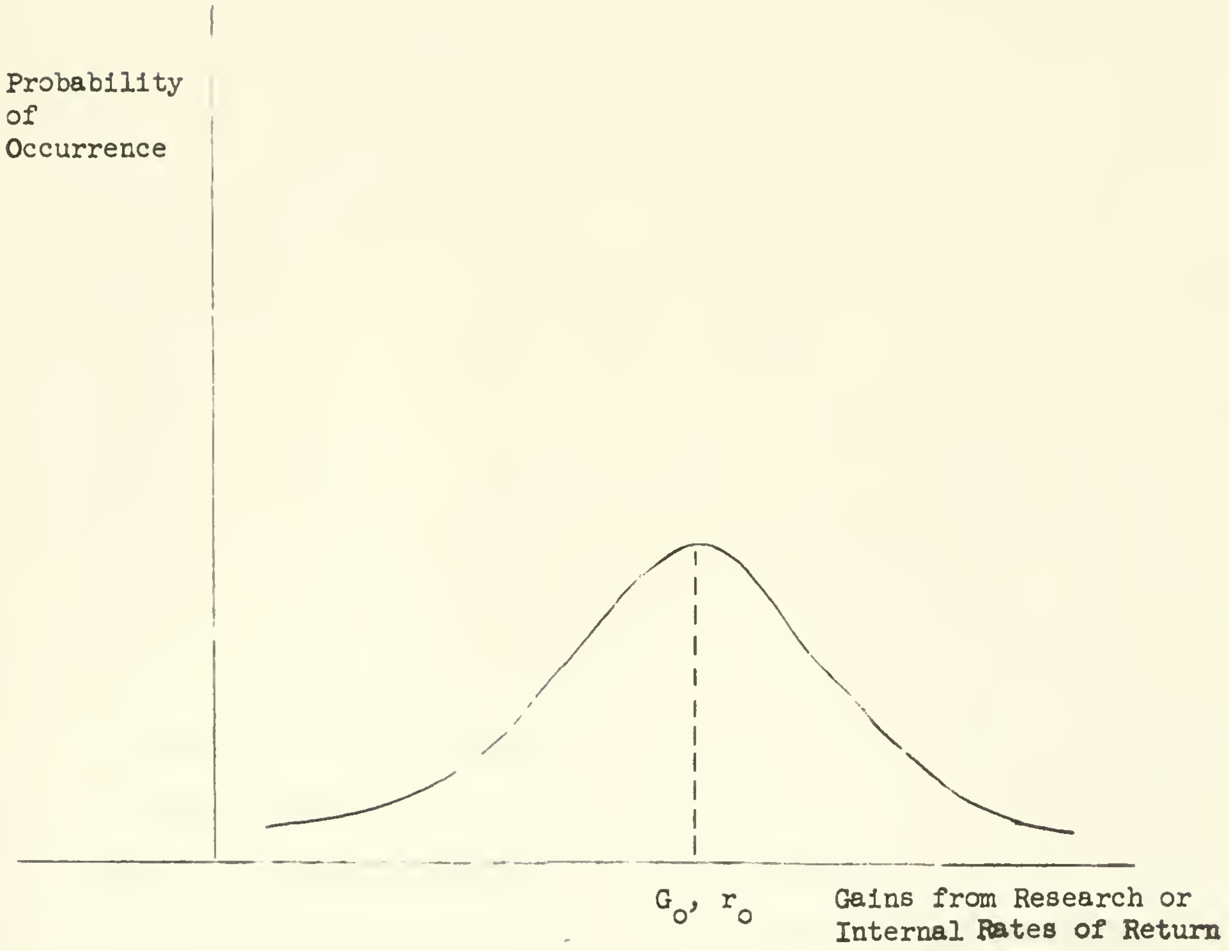
The identifying characteristics of research are that results from expenditures on this activity are not certain, nor are reactions to the results. Cost-effectiveness calculations do not show the effects of these attributes, but straightforward extensions of the preceding calculations can be made that do. The economics of investment decision-making sets out rules for deciding on projects based on characteristics of risk that can be used to rank "effectiveness" in other than dollar terms.

If the fruits of research include a new product, then demand is "unknown," in the sense that no fixed curve DD can be plotted as in Figure 1. The location of such a curve in the diagram has to be considered indicative only of one probable price-quantity relationship. There is some probability that P_o will clear the market of Q_o; there are other finite probabilities of both higher and lower prices for Q_o. The cost function is also unknown, in the sense that marginal costs at Q_o are MC as in Figure 1 with a certain probability, but can be twice as great or half as great as well. Then the shaded area in Figure 1 equals the annual gains from research output forecast to occur with a probability given by the product of the probabilities of P_o and MC. Other shaded areas, from the occurrence of other levels of price and marginal costs have other probabilities. The calculation of gains, but for each combination of conceivable prices and costs, results in a frequency distribution of results from research as in Figure 2.

This distribution is for gains from one year's adoption of the research results. Consider a series of such distributions, one for each year in the project's lifetime. The distribution for total gains from

Figure 2

Probability Distribution of Gains from Research



the project can best be constructed by finding the rate of return equalizing research costs and buyers' gains for each conceivable combination of such research costs and gains. The probability of any combination is the joint probability of that stream of annual gains and that total expenditure for research; adding together probabilities of the same rates of return results in a distribution of rates by probability. Figure 2 can illustrate

that distribution very well, with r_o the expected or average rate of return equalizing the series of shaded areas as in Figure 1 with the various possible total costs of research.

Each potential project has such a distribution, conceptually at least. Each project's worth can be described, as a first approximation, by the expected value, and variation from the expected value, in this distribution. Some projects can be said to be more "efficient" than others, on the basis of these dimensions: of all those projects with equal average rates of return, the project with the smallest variation in rates of return is most "efficient."[11] The investment rule is to choose "efficient" research projects on the basis of reasoning as follows. The results of research go to final consumers that have definite preference patterns: they prefer more to less, but additional satisfaction gained from another unit is always less than on the previous unit. Then the probability of rates of return times the satisfaction gained is small for high rates of return, but high for very low rates of return. The project with larger variance in the rate of return promises little satisfaction gained, and higher probability of large satisfaction lost. The average of satisfactions gained is lower for this project than for one with the same average rate of return but lower variance. This project should be rejected to maximize consumer satisfaction (whether such is ever measured or not).

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[11] The variation in many cases can be conveniently indicated by the "variance" in r equal to $\sum_{i=1}^{n} (r^* - r_i)^2/n$ where r^* is the expected value $\sum_{i=1}^{n} r_{i/n}$. Cf., H. Markowitz, *Portfolio Selection: Efficient Diversification of Investments* (New York: Wiley, 1959); and, for lucid examples of this rule in the choice of capital-budgeting projects, cf. G.D. Quirin, *The Capital Expenditure Decision* (Irwin, 1967), Chapters 10, 11.

Consider the choice of one of two research schemes, each of which is designed to provide marketable product "Q" by the end of a five-year development period. It may well be that the cost-effectiveness of each of the two is the same, so that both expect to cost $5 million in the one-year research period, and to provide a stream of gains equal to $5.61 million in the five years thereafter. Enough is known of the research problems in the two projects to say that the chances are equally as great that total research costs will be $400 greater or less than announced in the first project but not in the second.[12] The distribution of rates of return on the first project would center at four per cent, and have equally likely values at two and seven per cent. The distribution of rates of return on the second project would be one point:, four per cent with 1.0 probability. The second project has to be the avenue of research, in order to reduce variation from expected results as much as possible.[13]

The choice in another circumstance shows an unusual program from following the rule. The decision-maker has available to him the plans for four projects all designed to develop the same component; the projects all expect the same costs for producing this component once it

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[12] This is not to suggest that estimation of probabilities is a simple and straightforward task. As will be seen in the example of the breeder reactor projects, there are formidable problems in analyzing research plans so as to include the possibility of alternative results. The formulators of projects do not provide for variations from their plans. But some estimate of probabilities -- even of relative magnitude, as above -- is better than none, because consistent decision rules can then be applied.

[13] Given that less risk -- less chance of departure from design -- is better than more risk. Such an assumption seems to conform to observed industrial behavior in all except the most special cases. Cf. Quirin, op. cit., p. 203.

has been perfected, but each takes a different route to the final objective. An interesting first plan is to undertake all four projects for \$250,000 each and then choose one of these to complete the task (at negligible additional expense), while a second plan calls for putting the \$1 million into the most promising project from the beginning. The metallurgical and thermal energy control problems of these projects have led the decision-maker to conclude quite subjectively that the probability of success of each of the four projects is 1/4 and of failure is 3/4 -- except when \$1 million is assigned to one single project and the chance of success of this project is 1/2.[14] Then successful completion of the task after spending \$1 million altogether on the four projects has a probability of 6/10 [the probability of four failures if $(3/4)^4$, which is one minus the probability of at least one success], and after spending this amount on only a single project has a probability of 1/2. The dominant program is that which undertakes all four projects at low levels until one of the four proves out.[15]

The choice of "efficient" projects may not be sufficient to discriminate, however, if there are some not efficient relative to others. One project might have an expected internal return r_1 and variance S_1^2.

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[14] In each instance, complete failure is assumed to be the result of "lack of success." There is no partially successful project, so that "lack of success" does have a probability of 3/4 for each project.

[15] This case would seem to be an example following the appeal of B. H. Klein for duplicative projects as a cost-saving method of solving research problems. Cf. B. H. Klein, "The Decision-Making Problem in Development," in The Rate and Direction of Inventive Activity: Economic and Social Factors, National Bureau of Economic Research Report, Princeton University Press, Princeton, 1962.

The second proposed project has a larger expected value of returns r_2, and a larger variance S_2, so that it does not turn out to be clearly better than the first proposal. It promises more, but with more risk. A particular decision-maker may not have as adverse a reaction to poor results as another, so that he takes on the more risky project (with higher probabilities of inferior output, higher costs than called for in the design, et al.) for the promise of higher expected return. The Federal Government's reaction to poor results -- as a reflection of the average reaction of final consumers -- has to be decisive in the choice or rejection of the projects in question outside of any general rule.

Almost any research project is more complicated than the problems dealt with here by the simple argument for "efficient" investments. The project in many cases is a number of projects, each of limited scope and as one of a number of steps leading to a final product. Progress can be gauged by the success of the sub-projects, so that continuous assessments can be made of the overall worth of the research program. The choice is ever present between continuing a project already accepted, and abandoning this project. The choice is present, as well, between continuing the project on one hand and escalating it to obtain a higher quality final product (with greater final demand and greater research costs). But there is no reason for believing that these added complications require specific changes in the rule for "efficient" projects.

Rules for deciding in all circumstances whether to start a project, to continue it, or to up-grade it comprise a "strategy." The economic strategy in the more complex decisions on research still calls for

calculating the rate of return from each possible research result and finding the frequency distribution of the rates of return from (subjective) assessments of their probabilities. Strategy consists of choosing the project or project mix with highest average and lowest variance in the rate of return. This single rule produces most of the correct reactions to risk. It implies the choice of the ongoing project over an entirely new approach if the first has lower additional research costs than the total research costs of the new, and the choice of greater expenditure for a higher-quality output, if the additional net gains under the (higher) demand curve are greater than those from putting the additional research costs into a project elsewhere. But the reaction to the project with both higher average and variance in r is still a matter of the Government's evaluation of the satisfactions gained by the average final consumer from the first, and lost by the second. Some indications are possible in particular cases; strategy can only be proposed for nuclear research by working out the conditions for decisions on "efficient" and "non-comparable" projects there.

Strategy in Past and Future Nuclear Research

The next chapter lays the groundwork for economic decisions on the breeder reactor program by assessing the results from multiple projects in the past twenty years of reactor development. These results, both in terms of price-cost margins and of departures of research costs from original proposed costs, can be expected to be realized again. They set the framework for choice among competing projects through forecasts of

future price behavior (Chapter 3) and of departures from presently proposed research costs (Chapter 4). Initial forecasts of the probable gains from the three most prominent projects are attempted in Chapter 5. Then the frequency distribution of rates of return are evaluated in the last chapter, and the strategy for "efficient" projects is applied.

2. STRATEGIES FOR NUCLEAR REACTOR DEVELOPMENT IN THE 1950'S AND 1960'S.

Programs for developing steam generators using nuclear energy were started in the early 1950's -- perhaps much earlier, on an informal basis -- for quite obvious reasons. The goal of the early programs was clearly to develop a system from capital equipment, nuclear fuel, and labor which utilized energy from nuclear fission to produce high-temperature steam. The principles of fission energy had been known for some time: by striking the nucleus of a heavy atom with a neutron, so as to split this nucleus into two or more parts, a reduction in mass takes place which has to be accompanied by the release of energy in accordance with the partial differential of $E = MC^2$. But it was not practical to put this energy to use because fission could not be sustained, nor was equipment available for capturing a large portion of the resulting energy. During the late 1940's great progress was made on solving these problems, and since then the challenge has been to capture energy at costs less than those associated with that from boilers using fossil fuels.

Theoretical and Applied Research

For continuous energy release, fission must be self-sustaining -- neutrons must be released by "N" fissioning atoms which collide with "N" atoms that are also fissionable, or else the complicated and unwieldly task of continually inserting neutrons into the fissioning material has to be undertaken. Not every neutron released in fission strikes another atom; some escape from the material by a chance lack of collision while others form isotopes of no importance or

value. Then to ascertain that one released neutron collides with one more fissionable atom, a certain-sized volume of nuclear fuel is required. Few neutrons can escape, so that capture of neutrons by fissioning atoms is enhanced, if the fuel mass has a small surface area and a large internal fissioning area. To provide the best fissioning environment, this fuel mass or "core" should be as large as the state of the art will allow, because the escape surface is then the smallest possible in relation to the volume of fissioning material.

The fissionable atoms undergoing the greatest release of mass and energy are thorium Th^{232}, plutonium Pu^{239}, and the uraniums U^{233}, U^{235}, and U^{238}. The first and the last materials have a significant probability of fission only if neutrons collide with their atoms at high speed (with kinetic energy of more than one million electron volts) while the others fission with neutrons at all speeds but most probably with slow neutrons (1/10 of an electron volt). Then there is a choice of environments; the reactor can occur in a compact core of Th^{232} or U^{238} if fast neutrons cause fission, or in a diffuse core of U^{233}, U^{235} with a moderating material to slow down neutrons. The first is a fast reactor core, the second a thermal reactor core.

The choice of reactor core size and type depends on the state of research in applied physics. In the case of a design for a thermal reactor, the theory specifying core size required to sustain fission may exist, but in that of a fast reactor this work may not be done or may remain to be tested by laboratory experiements. As important as core size is the specification of that core content which allows heat production without losing control of the reactor. Surges of temperature and pressure, even if short-lived, might in some instances accelerate fission so as to render any outside limits ineffective; theoretical analysis of these instances must be complete enough to make it possible to avoid them.

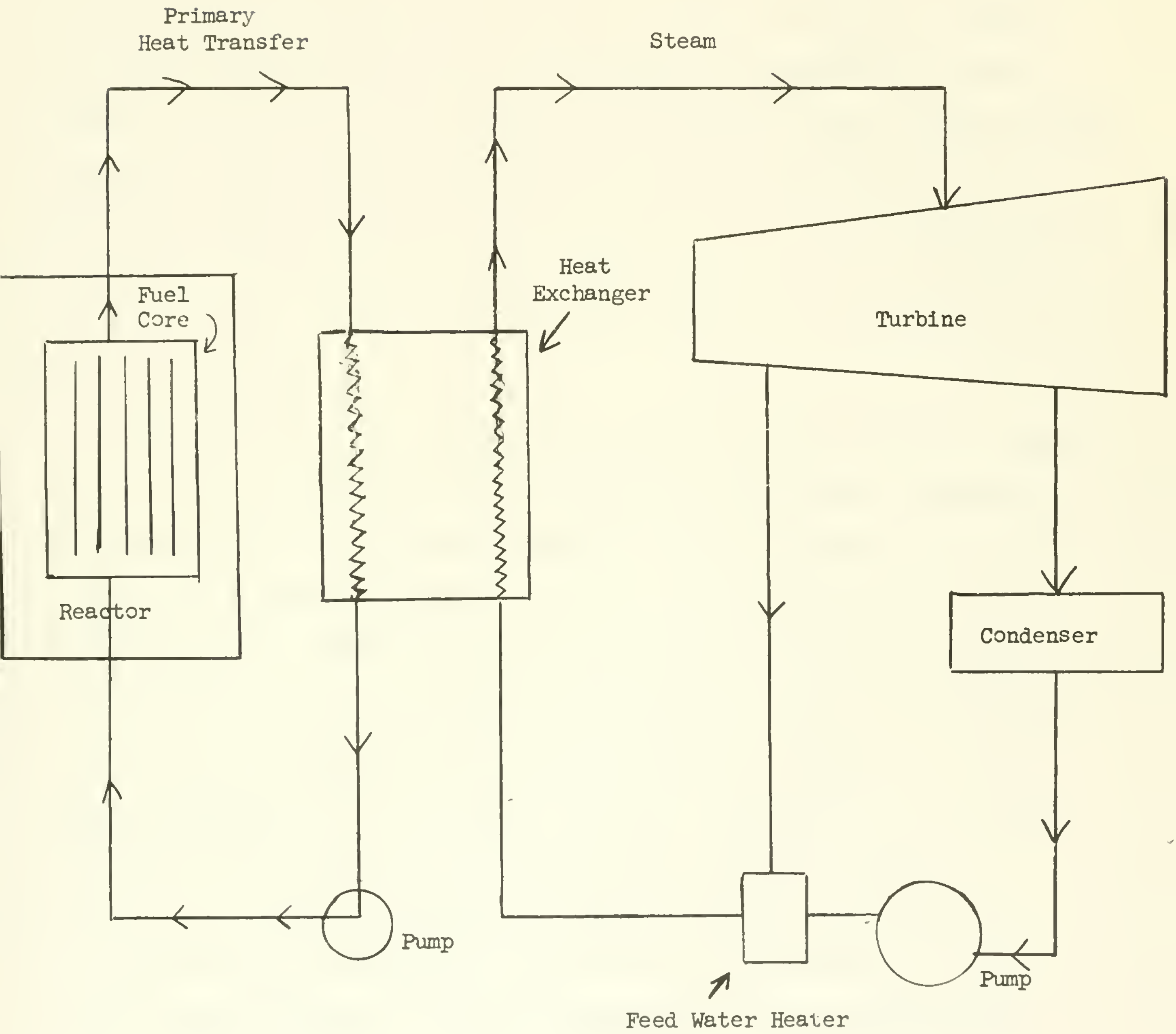
The choice of a reactor is broadened by the availability of a large number of potential transportation media for carrying heat energy from fuel core to steam generator. Each medium operates best under particular core temperature and pressure conditions, so that there is direct feedback from heat exchanger components to reactor internals. The routing of heat energy generally follows that shown in Figure 3: the heat-absorbing liquid or gas is pumped up through the core and then out of the reactor to a heat exchanger; here steam receives the heat, is moved to the turbine and subsequently to the condenser. The coolant in the primary routing can be a liquid metal such as sodium or a gas such as helium or even steam itself.

The choice here is a matter of the technology of materials. For the reactor with moderating material a_1 and coolant material b_1, the least-cost combination of capital components, fuel, and labor dictates the use of metals able to operate with b_1 at 500°F and 1000 p.s.i.g. But another reactor type with a_2, b_2 operates best -- with lowest costs for that type -- with b_2 at 1000°F and 100 p.s.i.g. The reactor types proposed in the late 1940's could not have been built and operated for long periods because there were not metals available that could withstand the stress and fluid corrosion of the first coolant at the first temperature and pressure, nor the corrosion and radiation damage from the second coolant under the second, higher temperature but lower pressure.

Research progress almost always called for the discovery and development of new metallic substances for reactor systems. These required experimentation with available but unproven substances, or new combinations of elements, under the specified conditions of temperature and pressure in a test facility built for a particular coolant. For sodium in a fast reactor core at 600°F and

Figure 3

Simplified Component Layout for a Nuclear Reactor



100 p.s.i.g., a test facility (termed Experimental Breeder Reactor 1, or EBR-1) was constructed in 1951 at the National Reactor Testing Station in Idaho; for water as moderator and coolant at 417°F and 300 p.s.i.g. in a thermal reactor, a separate facility was constructed in 1954 (the BORAX III). Experimentation has more often than not been a matter of trying materials termed "likely" as a result of past observations of alternatives under somewhat similar conditions. There has been little theory to guide the experimenter: "The properties of materials cannot be predicted from known natural principles, as engineering performance can be. This is the heart of the present materials problem for nuclear reactors ... experimental study is the only way to get the necessary answers."[1]

When solutions have been found both to the theoretical problems in neutron capture and to experimental problems in prolonging the lifetime of materials, then reactor systems have been put together. The first attempt to do so has usually been a reactor experiment which shows that the interactions of test components is not unstable. The plant may or may not have been more costly to operate than alternative systems; almost invariably costs are not mentioned in the single-minded pursuit of continuous operating performance. But then reactor prototypes have been built on the basis of experiments, and operated to continue the earlier work in the direction of reducing component costs and improving component performance. When these plants showed performance of components with costs similar to those for the same components in other reactors types considered to be at an "advanced stage", then demonstration

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[1] Dr. Russell Dayton, Battelle Memorial Institute, in testimony before the Joint Committee on Atomic Energy in Development, Growth, and State of the Atomic Energy Industry (87th Congress, 1961), p. 88.

reactors have been constructed. Success at this level showed to the final users of energy-producing systems that this reactor type could be chosen on the next round of construction of new commercial facilities.

Strategy in the Early 1950's

Collection of information on uranium reactions obviously began in the United States in the atomic weapons development program during the Second World War; even though most of it was not pertinent to reactor development, enough was related to the applied physics of the fuel core that reactor research could be turned to immediately in 1945. The weapons program itself was transformed into a Federal reactor development program: the national laboratories at Los Alamos and Oak Ridge took the initial steps towards reactors in the war research facilities by constructing "mock ups" there of self-sustaining fuel cores with heat transfer routings to steam generators. The bomb program was dormant, personnel were interested, and funds were available to keep the personnel in the facilities from drifting away. Both the resident scientists interested in continuing the relevant basic physics research and the personnel in laboratories and workshops for components development gave the Federal laboratories a strong lead in building the first systems.

There was nothing that could be called a "plan" or "strategy" for building operating reactors directing these early steps in experimentation. Projects of interest to the installed research groups were tried. Overall direction began with the organization of the Division of Reactor Development in the Atomic Energy Commission in February of 1949 and the first plan -- consisting of five major reactor projects -- was announced soon after. These were still based almost entirely on the interests and expertise of established research groups in national laboratories -- three of the projects were continuations,

the other two may have been on the basis of "demand conditions" separate from growing research interest.

The first two projects were supply-determined in the sense that previous laboratory research programs had produced test results in physics and working components which put them ahead in developing an operating prototype. The experimental breeder project had a lead at the initial stage of putting a system together because a Los Alamos experimental fast reactor termed "Clementine" constructed on breeding principles had been working since 1949. The same was true of the "homogeneous reactor experiment": an advanced prototype was to be constructed as an extension to a program underway at Oak Ridge that had demonstrated circulation of fuel, moderator and coolant together through a heat exchanger.

The other two projects were demand-determined, in the sense that they were attempts to produce new reactor types even where there were no existing experiments. A small-sized reactor with appreciable power was required by the military for submarine development. Regardless of limitations in data then available in thermal core physics, a "submarine thermal reactor" was to be constructed because slow neutrons were easier to control in that environment. The last reactor -- an operating core and heat transfer mechanism to test materials -- was needed to overcome materials stress and corrosion in the other three projects. It was derivative from the other three, but only in specifics of the design since a general test facility would have been constructed for most other possible projects. The four projects comprises an ad hoc plan: choose reactor concepts that are ahead in parallel experiments in the national laboratories, fill-in between these projects in materials sciences, and accede to military requirements.[2]

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[2] For more description of these projects, cf. U. M. Staebler, "Objectives and Summary of U.S.A.E.C. Civilian Power Reactor Programs," The Economics of Nuclear Power (J. Gueron, et.al., New York: McGraw Hill Book Company, 1957).

Research under the ad hoc plan was not the only work done in the early 1950's, however. There was an extensive program not related to specific reactor concepts but consisting of a large number of smaller experiments with fuels or capital components in simulated reactor environments. Coolant experiments with boiling water at the Argonne National Laboratory, and studies at other national laboratories of radiation damage to metals and to other, more erratic coolants provided basic knowledge for test reactors but was not specific to any project begun at that time.[3]

In February of 1954 a formal plan was set up to make additions to and subtractions from these ad hoc Atomic Energy Commission projects. Research and development had to pass through three stages: (1) problem solving in basic physics or metallic alloys, (2) the construction and operation of "first-of-a-kind" or prototype reactors to show the technical feasibility of systems, (3) the construction of demonstration reactors to generate electric power at costs comparable to those from alternative fuel sources. The first two stages were to be in the province of the AEC, with Federal support of both national and private laboratories for the accomplishment of the basic and applied research, and with Federal expenditures for the development and construction of the first-of-a-kind reactors. The private companies that manufactured reactor components, or that produced electricity, participated at the second stage and provided most of the third stage. These corporations took part in first-of-a-kind reactors through "buying the steam" -- either by constructing the steam and electric generating facilities of the system or actually purchasing steam from an AEC-owned reactor.

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[3] Cf. U. M. Staebler, op.cit.

The corporations were to construct the third stage reactors themselves with AEC assistance of unspecified nature and extent.[4]

The plan itself was to build five first-of-a-kind reactors for predesignated levels of costs. The five were first steps on divergent paths to economic power capacity -- some based on forecasts of low capital equipment expenditures but an inability to utilize fuel so as to compensate for high uranium prices, while others were based on high capital costs but low fuel prices to produce the cheapest power. Each faced formidable technical problems which had to be solved before steam could be generated for a reasonable length of time.

The fast breeder reactor program, which began with EBR #1 in 1951, was to be expanded with the construction of EBR #2 in 1956-1958. This second reactor was substantially a "first-of-a-kind" because the increase in its size over EBR #1 raised formidable new research problems. Capacity was increased from 1,400 thermal kilowatts in EBR #1 to 62,500 kilowatts,[5] and the internal temperature of the primary heat transfer loops was raised from 600°F to 1000°F. The first was not a simple task given that the larger and more numerous components had to be of perfect tolerance to prevent fouling and leak proof to prevent explosion of the

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[4] Staebler states that industrial power reactors at the third stage were to be "fully justified on an economic basis" which would imply no need for AEC assistance; but this stage was part of the AEC program, so some assistance must have been intended. Since no reactors went this far, these funding questions did not arise. Cf., U. M. Staebler, op.cit.

[5] The thermal capacity of the system is the total quantity of heat, in hundreds of calories per kilogram, in the steam per unit time. This is equivalent to the number of kilowatts of electrical capacity in the system if each calorie of heat can be completely utilized in the turbine generator without (a) loss before entering the generator, or (b) rejection of heat by the turbine to the condenser. Of course, this is not the case. If the efficiency of the system is, for example 35%, then the electrical kilowatts equal thermal kilowatts times 35%. Cf., for example, G. Yekholodovskii, The Principles of Power Generation, (Pergamon Press, 1965), p. 91, et.seq.

liquid sodium coolant on contact with water. The higher temperature was necessary to attain the capacity/fuel ratios required for long run economic feasibility, but the resulting level of neutron acceleration was so high that the fission reaction sequence was difficult to control;[6] at the least, physics research predicting the response patterns to temperature changes remained to be done.

The homogeneous reactor program was also to be expanded, and in a manner quite similar to that for fast breeders. The Oak Ridge research group had constructed HRE #1 in 1952 under the first AEC reactor program, and had operated it for almost two years before the 1954 five-part plan was under consideration. There was sufficient experience to show that fuel, moderator and coolant could be combined and circulated through the coolant loop while still containing most of the fission in the core.[7] That is, a primary coolant loop and heat exchanger could be built which was tight enough to prevent leakage of radiation throughout the reactor. But the question whether the primary system pressure could be raised from 1000 p.s.i.g. in HRE #1 to 2000 p.s.i.g., a level more in keeping with efficient fuel utilization, remained unanswered by tests in HRE #1. The development of coolant pumps and leak-proof components in a higher pressure system required experiments and tests of mock-ups similar to those previously at Oak Ridge but of another order of magnitude in expenditure on materials and personnel. Corrosion and radiation damage still disrupted the operation of the first HRE; the circulation of uranium sulfate at 2000 p.s.i.g. magnified the possibilities of extended

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[6] The general complaint was that, if the sequence from neutron to atom to neutron was disturbed by the temperature surge which increased neutron acceleration, then more fission would occur than previously. This would raise the temperature in the reactor core, which again would increase fission, so that any deviation from the original sequence, from random temperature variations, would not dampen down. This problem, while it did not arise in fact in the question of EBR #2, determined the course of the research in fast breeder reactors in the 1960's.

[7] The core consisted of little more than a "bulge" in the primary loop large enough to sustain fission.

disruption so that research had not only to continue in order to solve old problems but to begin anew with new materials in anticipation that the old problems would be much greater in the new environment.

The submarine reactor program had produced a working system for generating steam from fission. Distilled water under pressures of more than 2000 p.s.i.g., used as both moderator and coolant, proved to have good heat-transfer properties and to provide sufficient moderation for generating built-in limits on the rate of fission reactions. The system thus "worked" both in the sense of transferring heat from fission to a steam turbine according to design and in being able to return to design fission behavior if a random temperature or pressure change took place. Then the goal was to improve performance -- to lengthen the lifetime of the fuel and reduce the amount of fuel required for a thermal kilowatt of capacity so as to make this sytem comparable in cost to fossil-fueled steam generators. Towards this result, a pressurized-water reactor program was established to build non-submarine prototypes. The new program was to rely on the results achieved so far with the submarine program, but to go further to "demonstrate that relatively large pressure vessels can be built according to specifications required for reactor operation ... and (develop and) control very closely spaced fuel elements.[8]

The pressurized-water reactor cycled H_2O under high pressure in liquid form; one alternative was to run at lower pressure and circulate steam through the reactor directly to the turbine generator. This was an attractive possibility because it eliminated much of the capital equipment for the pressure vessel (which

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[8] Abstract of Unclassified Material from Classified AEC Report to the Joint Committee on Atomic Energy, "Program Proposed for Developing Nuclear Power Plant Technology," (received by the Joint Committee on Atomic Energy, March 12, 1954; cf. Report of the Subcommittee on Research and Development on the Five-Year Power Reactor Development Program Proposed by the Atomic Energy Commission, U.S. Government Printing Office, Washington: 1954).

might be 6 to 8 inches thick for the higher pressure P.W.R.) and because it eliminated the equipment in the heat exchanger entirely. But development was no more complete than for the homogeneous reactor, nor as complete as for the pressurized water reactor in the submarine program. No working system had been constructed, even at the experimental stage, so that the first boiling water reactor had to be an experiment to "determine whether it can be operated without significant deposit of radioactivity in the turbine, the condenser, and the feedwater pumps (from the steam leaving the fuel core)."[9] There was no expectation that this project would lead directly to a demonstration plant: "EBWR (Experimental Boiling Water Reactor) is not expected to provide nuclear data on the critical mass of a large reactor or on proper spacing of its fuel."[10] Another stage of development beyond that planned in this five-year program was required before blueprints for economic boiling water reactors could be given out.

The fifth project in the plan was to build a reactor experiment with liquid metal (sodium) as the coolant and graphite as the moderating material. The sodium graphite reactor was not a direct descendant of the early post-war experiments in the national laboratories but rather was cross-bred from a number of such experiments. The breeder and other sodium research had demonstrated the heat-transfer capability of sodium (as well as the problems to be encountered in heat exchanger leaks from sodium burning in air or water), and small-scale experiments with graphite in the National Laboratories had shown high quality performance as moderating material. Independent design studies of sodium graphite systems (by Atomics International Company) were added to the experimental work; the sum total of

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[9] Ibid.

[10] Ibid.

promising results and designs led to the decision to take on this reactor concept in 1952. There was much to be accomplished by building an experimental plant: "Many features of the plant, and its operating procedures, have not been tested in reactor practice ... the upper limits for fuel and coolant temperatures, burnup and other operating variables ... moderate changes in these variables such as increasing maximum uranium metal temperature from 1200°F to 1400°F and maximum coolant temperature from 1000°F to 1250°F could have an appreciable [reducing] effect on the cost of power."[11]

The final goal of all five reactor programs was to demonstrate the technology necessary to build steam generating capacity at $50 to $70 per thermal kilowatt (or $200 per electrical kilowatt). There was widespread confidence that the relative abundance of uranium over fossil fuels such as petroleum and natural gas would make nuclear fuel less expensive; but there was less confidence that nuclear capital costs would be comparable to those for the much simpler fossil-fuel-burning boilers: "The problem of developing nuclear reactors for the economic generation of electric power is largely one of reducing costs for capital investment ... for practical nuclear power plants of the future, construction costs of $50 - $70 per KW of heat are sought."[12]

The goal was not attainable within this program by each of the five research groups building reactors. The pressurized water program called for a large reactor (264 thermal megawatts)[13] at Shippingport, Pennsylvania, which would be the first "full scale" plant and would be expected to demonstrate that one additional round of components improvement would result in economic nuclear

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[11] Ibid.

[12] Ibid.

[13] A thermal megawatt is 1,000 thermal kilowatts or 1,000,000 thermal watts.

power. The four other programs were frankly experimental; they had to show that reactor concepts could be built and could be operated at least 80 per cent of the time after construction was complete.

The first project was expected to cost $800 per electrical kilowatt,[14] while the others were to cost from $600/KW to $1,000/KW. They were constructed according to the experience shown in Table 4. Shippingport proved to be operational at 50 per cent more capital expenditure than called for in the original design, but at about the time originally planned. The four experimental reactors had techni-

Table 1

The Reactors in the 1954 Five-Year Program

Reactor Type	Plant	First Predicted Costs	Actual Costs
Fast Breeder	EBR #2	$740/KW (1957)	$2,320/KW (1961)
Homogeneous	HRE #2	$1,050/KW (1956)	$1,650/KW (1958)
Pressurized Water	Shippingport	$800/KW (1957)	$1,250/KW (1957)
Boiling Water	EBWR	$900/KW (1957)	$1,180/KW (1957)
Sodium Graphite	SRE	$630/KW (1956)	$960/KW (1957)

Source: Atomic Energy Commission reports to Congress on the Civilian Nuclear Power Program (most frequently before the Joint Committee on Atomic Energy, Hearings pursuant to Section 202 of the Atomic Energy Act of 1954, from 1960 to 1967).

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[14] The capital costs on this page and those following in this chapter are quoted in terms of dollars per electrical kilowatt -- equal to total expenditure on all items of capital, on interest charged during construction, and on research and development expenditures specific to this plant, all divided by the total net electrical capacity of the plant. This departs from the emphasis in the preceeding pages on production of heat energy or thermal kilowatts. The capital costs per unit of heat energy can be estimated by multiplying these statistics by plant efficiency, usually of the order of 30 to 40 per cent.

cal difficulties of different degrees of seriousness. No single experiment was constructed with capital costs equal to the original design expenditure; and the total expenditure averaged almost twice that for the original design. All took somewhat longer than expected to put into operation: one was completed late in the year originally forecast; two were one or more years late, and the last was completed four years behind schedule.

The five-year plan was put into effect and its goals were realized to a limited extent. Shippingport proved the point it was supposed to prove, in part: although it did not demonstrate power capacity at $800/KW (including expenditures on original research), it showed well enough that the next round could take place at a lower level of costs. Two private PWR power reactor projects were announced subsequent to the first few month's operation, and both had forecast capital costs in the $200/KW to $300/KW range. There was no assurance from Shippingport's experience that these costs would be attained, or that fuel costs were "negligible"; but the broad outlines of cause and effect in the demonstration project were being followed. The experimental projects were successful to a more limited degree. Three of them resulted in working reactors in the five years of development, albeit at higher cost levels than expected; but one had not yet begun operation by 1958, so that only three out of four had "proven out" within the standards set by this program.

Planning and strategy are difficult to tell apart at this very preliminary stage in reactor development. The five-year plan set out the goals for three stages of development, and also set out the five projects to attain either the first or second stages, without hinting at changes which would take place if the first stage goals were not reached. The expenditure goals were not achieved

nor were scheduled dates of completion generally met, but projects were not curtailed. Then the plan must have been the strategy: the five projects were to be continued, even if there were significant departures from expected results.

Strategy and the 1958 Ten-Year Program

A draft statement of a long range plan for the development of civilian nuclear power was published by the Joint Committee on Atomic Energy in August of 1958.[15] The goals of the program, to be reached before 1969, were centered on producing "competitive power in high cost energy areas." Both goals and the means for achieving them were specified in some detail; they showed a tendency for AEC program planners to move further in the directions already laid down in the previous five-year program.

Competitive power from a reactor system was said to be available when "utility executives [could] make a decision to build nuclear stations on the basis of lower costs over the life of the reactor."[16] Not all utility companies were expected to be able to make such a comparison, but rather only those in "high cost energy areas" where electricity from fossil fuel systems were "estimated to be [available at] 7 mills per kilowatt hour."[17] These costs were to be met by combinations of nuclear fuel expenditures of 2-4 mills per kilowatt hour and capital expenditures in the range of $100-$300 per kilowatt of capacity.

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[15] Cf. Proposed Expanded Civilian Nuclear Power Program (U. S. Government Printing Office, Washington, August 1958).

[16] John A. McCone, testimony before the Joint Committee on Atomic Energy, Hearings Pursuant to Section 202 (February 16, 1960; pp. 122, et.seq.)

[17] Ibid.

The exact target depended on assumptions on the percentage of time that any one reactor system would operate, on prices per unit of fuel and of capital, and on interest charges; the approximate target was to undercut fossil fuel costs by more than half, so as to make it possible for nuclear capital costs of $200 per kilowatt to be competitive. This target reaffirmed that of the 1954-1958 program, as far as capital costs were concerned; but it extended beyond that in the earlier plan by making explicit the necessity to reach fuel costs as low as 2 mills per kilowatt hour.

The research projects were also extensions of those in the first program. A number of routes were to be taken to demonstration reactors -- all beginning with experiments on reactors components, continuing with construction of "first-of-a-kind" small scale reactors, ending with construction and operation of full scale plants showing the attainment of target costs. But there were to be more such steps in this sequence and more reactor types at each step.

First-of-a-kind reactors were to be built in two sizes, one at a small scale for experiments with original components and newly-developed materials in simple systems (termed "experimental power plants"), the second at full scale for collection of information on the behavior of these componenets at useful size (termed "prototype power plants"). The second-sized experiment was an additional step in development, given that "advanced prototypes" and "demonstration reactors" completed a project.

More reactor types were to be made the subjects of research than ever before. The original five reactor programs were to be continued, with the four preliminary programs going to larger prototypes, and the advanced program to both an additional, very advanced prototype and to demonstration reactors.

Large-scale prototypes were to be added to the *fast breeder*, the *homogeneous*, the *boiling water* (where three new prototypes of various sizes were scheduled), and the *sodium graphite* reactor projects. The prototype plant at Shippingport was not to be terminal development in the *pressurized water* reactor project, but was to be supplemented with three newer reactors which would attempt to improve performance, mostly by adding to steam temperatures and pressures. Three new projects for new reactor types were added to the original five at a level of research activity quite comparable to them: one project using an *organic coolant* (which had been operated at a lower level for some time, outside the five-year program); a second project for a reactor using *heavy water* as both coolant and moderator (based mostly on promising experiments in Canada); and the *gas cooled reactor* project, expanded from what had been a number of private ventures with helium cooling by one of the private reactor manufacturers. The entirely new projects were to start up with small experiments, and to continue through to the final demonstration reactor. By Congressional count, "present plans call for design studies during the next five to seven years of 21 reactors, including nine of large size, four of intermediate size, three of small size, and five small experiments."[18] Not all of these designs were expected to become working plans for plants, but fourteen were to be built for expenditures of $.4 billion for plant and equipment and of $1.3 billion for general research.

The strategy for effecting the plan consisted of some working rules as to choice of actual projects when the time had arrived to begin construction.

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[18] Cf., "Comments of Reactor Designers on the Proposed Expanded Civilian Nuclear Power Program," *Joint Committee on Atomic Energy*, 85th Congress, December 1958.

There were more designs than construction projects: "about half of the most promising designs would be carried through the construction stage ... (with) total capacity of plants to be built in this country ... expected to amount to about one million electrical kilowatts."[19] The choice of few plants from many designs was to be made mostly within reactor types -- for example, four designs of a reactor type might precede the construction of one small scale experiment of that type -- but implicit in the program was some comparison across types, and a mandate to eliminate any of the parallel projects that fell considerably behind the others.

The strategy actually put into effect over the ten-year period was to cancel the advanced steps in any project that appeared to be "far behind". All projects failed to stay within predesignated levels of capital costs; as shown in Table 2, the average ratio of actual to design costs was greater than 1.5 for all projects. But those coming out of the previous five-year program at a fairly advanced stage of development -- beyond the small-scale experiment stage into that of construction of large-scale prototypes[20] -- can be divided into three classes. The first class projects showed costs 20 per cent to 40 per cent greater on average than expected, but a tendency to approximate design levels of costs more closely in later reactors. These projects, the two light water projects above the first line in Table 2, were advanced through each of the

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[19] Ibid.

[20] The fast breeder program was still at a much earlier stage of development. Although small experimental reactors were in use or being constructed (such as EBR-1 and EBR-2) and a full-sized prototype was under construction, standards for reactor operation were different from those applied to other projects. The time-length of development was to be much longer than for other reactor types, because the optimal time for installing breeders appeared to be in the late 1970's or early 1980's. The expected costs of the program were much more extended, as well, and those incurred at this stage were only preliminary to a much larger program. As a result, immediate availability of a working economic reactor was not required for this program.

Table 2

Forecast and Realized Costs for the 1958 Ten-Year Program

Reactor Type	Ratio of Realized to Forecast Expenditures on Construction, for all Reactors of that Type Completed Before 1967.	Number of Reactors
Pressurized Water Reactor	1.2	3
Boiling Water Reactor	1.4	5
Heavy Water Reactor	1.7	1
Gas Cooled Reactor	1.9	1
Sodium Graphite Reactor	2.4	1
	> 2.0	Cancelled
Homogeneous Reactor	1.6	1
	Unknown	Cancelled
Organic Cooled Reactor	2.1	1

Source: Various fiscal reviews of the civilian power program, from annual publications of the Atomic Energy Commission and the Congressional Joint Committee on Atomic Energy.

Note: The estimates for the Fast Breeder Reactor are not shown, for reasons given in the preceding footnote.

designated stages until they achieved the program target of demonstration reactors costing $200 per kilowatt of capacity. The second class projects showed costs 70 per cent to 90 per cent greater than prescribed at the stage of construction of fairly advanced experiments; the absolute levels of these costs were very high, as well, since they exceeded $1,350 per kilowatt. The two reactor types in this class, the heavy water and gas cooled projects below the first line in Table 2, were put on a "contingency" basis by making later stages of development depend on and wait on the performance of the two experimental reactors which were under construction in the early 1960's. The third class projects were those that were cancelled. Two of them, the sodium graphite and organic cooled projects, had shown costs twice as much as expected; while the homogeneous reactor experiment had costs only 1.6 as much as originally forecast, it was able to provide capacity only at $1,650 per kilowatt. None of these in the last group had provoked private developers into plans for cooperative prototypes, because other projects already had lower forecast energy costs at the same or a more advanced stage of development. These were reasons for cancellation given by Mr. Milton Shaw, Director of the AEC's Division of Reactor Development, in his discussion of the sodium graphite project: "The utility interest is not there; the economics are not there in terms of positive factual evidence to date ... the resources put into this plant thus far in terms of sodium technology could better be applied to the liquid metal fast breeder program."[21]

The strategy was in fact to choose from the ten-year program those particular types that had lower forecast energy costs and the better record of cost

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[21] Testimony of M. Shaw, Hearings Pursuant to the AEC Authorizing Legislation Before the Joint Committee on Atomic Energy, February-March 1966, Part II, p. 819.

performance on construction of research reactors. This was a matter of fact, since from among eight projects -- all going through two of four steps to demonstration reactors -- two thermal projects were funded to the final stages because they had managed at the early stages to pull ahead of the others. These two had quite similar construction costs, so that choice between them was of second-order importance; they had costs per kilowatt of capacity which came closest to design costs, and they had lower construction costs at each particular step in development.

The Results of the Ten-Year Program

By the standards of measurement set up in 1958, the ten-year program was a success by 1967. Two of the eight reactor projects had achieved the goals set for capacity costs -- more than achieved these goals, since capital expenditures in new designs for light water reactors had fallen considerably below the $200 per kilowatt that was sought. The development pattern was most striking and quite similar in the two reactor projects; both showed significant progress quite early in their respective programs.

The pressurized water reactor, which had been shown to be operable in the Shippingport case, displayed rapid and substantial improvement in cost and operating performance in the experience with the Yankee Atomic Electric Plant. This plant, put into operation at Rowe, Massachusetts, in the middle of 1961, showed completely reliable production of electricity from that point on; except for refueling, the plant remained in full operation and after the first refueling its rated capacity was raised 10 megawatts beyond the 155 megawatts expected. The Yankee design called for more capital expenditures than were actually made,

and actual capital costs were close to \$300 per kilowatt. This was a decrease from Shippingport of more than \$900 per kilowatt, so that the Shippingport experience along with further research expenditures of \$5 million in Yankee had brought the project more than 80 per cent of the distance to the \$200 per kilowatt goal. Three additional reactors of the pressurized water type were undertaken by private developers with AEC research grants[22] around the time that Yankee began operation; even though not all of them have yet been finished, they proved to be the last round of research. More than 15 private projects have been initiated since 1964 but previous to the middle of 1967 with scheduled construction on the basis of capital expenditures considerably less than \$200 per kilowatt of capacity.

The outlay on this project, and the results therefrom, can be characterized by taking a macro view of the construction of experimental, prototype, demonstration and operating electricity-generating reactors. This view of research is that, for a year's research outlay, a year's improvement in performance is obtained: $\pi_t = f(E_t)$, where π_t is a performance index on which to measure research progress, and E_t is an expenditure index, both occurring in year "t". For the pressurized water reactor project, π is design capacity costs of \$200 divided by the lowest costs actually achieved on a working PWR in each of the years 1957-1965, and E is the per cent the research expenditure in each year is of total 1957-1965 PWR expenditure. The function $\pi = f(E)$ that fits the nine years of experience best is the least squares equation $\log \pi = -0.249 + 1.28 \log E$,

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[22] The terms of modified "third round" assistance were that, if the companies built and operated the reactor systems, the AEC would contribute an amount to be negotiated for R&D expenses, and would provide the fuel core without charge. This must have placed the impetus on reducing capital charges rather than fuel costs since -- in effect -- other inputs besides capital were available without cost.

with the coefficient of correlation $R^2 = 0.876$; it indicates that results increase at a faster pace than expenditures (since the coefficient showing $d \log \pi / d \log E$, or percentage change in π/percentage change in E, is +1.28). This may be somewhat misleading, since a great part of the early experimental PWR work was done under the aegis of the submarine thermal reactor program and is not included in E; but, as a first indication, it would appear that the appreciable progress already made at the beginning of the project increased at a substantial rate with only small additional expenditures.

The successful completion of the project to prove out a *boiling water* reactor was not achieved with the assistance of basic research from a military program. But the similarities of results in this case and the accomplishments of the PWR program are still quite strong. The large step for the PWR project in the installation of Yankee Atomic was paralleled for the PWR project by private construction of Dresden #1 at almost the same time. Dresden showed that the *boiling water* concept could be put into effect for reliable power production -- in 1964, for example, the plant had a forced outage rate of only 2.2 per cent of total available operating time -- but the costs of construction were planned at $200 per kilowatt and realized at $340 per kilowatt. A major round of advanced development was still required, if the target of $200 per kilowatt was to be reached.

The approach to advanced development differed somewhat from that in other projects. Three prototypes were constructed as joint AEC-private utility research reactors, all in the range of 20 to 70 megawatts of capacity, each to center attention on solving a different problem. The Elk River reactor, with a closed cycle of water coolant from reactor core to turbo-generator and back, was completed in 1962; the Big Rock Point reactor, to demonstrate operation of

new types of fuel with extremely high thermal energy per unit volume, was completed the same year. They provided new and important leads to reducing the costs of the heat transfer system and fuel burnup respectively. But these two plants did not themselves show lower capacity costs than in Dresden #1; their contributions lay in providing better components or fuel for later reactors, so that little was achieved immediately from large additional expenditures for research.[23] The next round of construction after this achieved the target, without any further research expenditures: plants at Nine Mile Point and Oyster Creek were finished in 1967 with costs of construction of less than $200 per kilowatt, and were accompanied by announcements of more than a dozen orders for BWR plans of 500 to 1,065 megawatts capacity by private utilities in the 1965-1967 period.

This experience with research prototypes strongly affected the pattern of results per dollar of expenditure. For the least squares log regression, $\log \pi = -0.250 + 0.531 \log E$ and $R^2 = .622$; the curve does not "fit" the nine years of observations of percentage results π and percentage expenditures E very well, since the coefficient of correlation is only .622 for a very small number of observations. But it indicates as a first approximation that diminishing returns to research expenditures were predominant -- on average, an additional percentage point of expenditure brought the project 0.531 of a percentage point closer to target that year -- and this can only be attributed to the large outlays with no reductions in capacity costs during the middle years of the program.

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[23] The third prototype, the LaCrosse Boiling Water Reactor, was constructed to carry on experiments in the use of mild alloy steels and to develop advanced steam separators. It was not completed until 1967, so that it contributed little to the ten-year program discussed here.

The two reactor projects "on target" should not be greatly differentiated because of special circumstances in each. The general description of a successful project might well center on $\pi = f(E)$ for 18 observations, nine for the PWR and nine for the BWR in the years 1957-1965, so that the least squares equation that fits the combined experience is $\log \pi = -0.327 + 0.731 \log E$ with $R^2 = 0.557$. This derived relation fits better than that for the nine observations of the BWR, while it is no worse a fit than that for the PWR, so that there is some net gain from putting the two sets of observations together.[24] This summary view shows diminishing returns to research expenditure, since the average movement towards the target was only 73 per cent of the annual additions to expenditures (that is, $\partial \log R / \partial \log E = .731$). Even the successful projects experienced declining relative results soon after the experimental mock-ups were completed.

Lessons for Strategy

The success achieved in the two water reactor projects, and the contrasting lapses experienced in four other projects, would seem to be the source of two important pieces of practical wisdom. The first has to do with deciding on the numbers of research projects, when the important consideration is the total amount of research expenditure, and it prejudices the case towards more projects rather than less. The second is pertinent to the same decision, but on another aspect: attaining the greatest possible spread of benefits from nuclear research to the consumers of electricity. This finding also favors the larger number rather than the smaller number of projects.

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[24] The test here for "goodness" of fit is simple and direct: assume that the correlation coefficient is different from zero as a result of chance (so that there is no fit); if the chance of this happening is less than 2/100 abandon this assumption for the contrary that there is a significant fit. The first assumption cannot be abandoned, on these standards, for the BWR; it can for the PWR and the combined sample of BWR and PWR observations. Cf. Table V-A in R. A. Fisher, Statistical Methods for Research Workers (Edinburgh

There is first some indication that more research projects can be cheaper than less research projects, "per unit" of research results. This follows from two aspects of the experience in the ten-year program: all prototype construction projects exceeded cost forecasts, and not all projects achieved the program target after reasonable additional expenditures beyond those originally called for. The average expenditure, on those reactors actually constructed in all of the projects, was 180 per cent of the original amount specified. The additions made to the two light water projects were enough to reach the $200/KW goal set for private construction costs, and further "unexpected" expenditure might have brought the other reactor types to this goal as well.

The question is "how much" further expenditure. One conceivable answer follows from assuming that these other reactor types could have achieved the target by following the research pattern $\pi = f(E)$ set by the two successful projects, and that they were at E_o implied by realized π_o at the time they were downgraded. This is to say that all projects were on the path $\{\log \pi = -0.321 + 0.731 \log E\}$, but that the five lapsed projects were at the wrong points on the path; for one, the sodium graphite project was observed to be at $\pi_o = .20$ and $E = .82$ in 1963, but -- for that level of achievement -- was actually at $E_o = .41$ of total expenditure for a successful project.[25] Then the downgraded projects would have required expenditures in the middle 1960's roughly E/E_o times those originally expected, in order to arrive at a point on the path $\pi = f(E)$ to successful research results. These expenditures would have been necessary to build successful prototypes; since each prototype probably would

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[25] That is, for $\log \pi_o = \log (.20) = -0.321 + 0.731 \log E_o$, then $E_o = .41$.

have cost more than expected -- as had been the case on all projects -- then the total cost of taking any one project to completion, as a ratio of original expected outlays, would have been the product of E/E_o and the ratio of realized to forecast construction costs on each reactor shown in Table 2.

The relevant choice in retrospect was between a strategy of selection of promising projects from a number of parallel projects, and a strategy of "sticking to one's last" with a single reactor type.

The results from the first strategy are indicated by actual events: two successful design types for the costs of close to six projects (the expenditures on the five downgraded projects came to 4.4 times the average of the original forecast expenditures per project; with these LWR costs, an outlay equivalent to that for six projects was incurred).[26]

The results from the second strategy would have varied from case to case, depending on which single reactor system was chosen before the ten-year program began. If the PWR had been chosen, then one successful design would have been attained for 1.2 times the original forecasts costs; on the other hand, if the homogeneous reactor concept had been chosen, it seems most probable that the costs of success would have been more than seven times those originally forecast for that single project alone [4.5 times the original costs, so as to move onto the successful growth path $\pi = f(E)$, multiplied by 1.6 times the forecast costs for each reactor of this type constructed]. It might be assumed that the promise of success was about the same for each reactor type before 1960; although there was a military PWR reactor working, the chances of economic success with that

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[26] This once again ignores expenditures on fast breeder reactors since it is assumed that the results of the fast breeder project are not yet known.

concept might have seemed no greater at the time than with the six other types not yet to the prototype stage. Then the results from the single project strategy would have been the average of results from pushing each of the separate types to a final conclusion (where each of the separate results was E/E_o times the ratio of realized to forecast construction costs). This average after the fact is close to 3.5 times original forecast costs, so that it would have taken the cost equivalent of 3.5 original projects to prove out one concept by centering all attention on that one concept.[27] Then in order to develop two reactor concepts, (1) start out with six concepts and discontinue all except the two best after some experimental results with prototypes for each; (2) start out with only two concepts, and spend an amount on average equivalent to forecast costs for seven projects before these two reach the target.

The second lesson is that more projects mean more companies producing reactor systems, and this might well result in faster diffusion of the fruits of innovation. The first part of this lesson is the easier to believe, because the results of the ten-year program showed that the manufacturer constructing the experimental and advanced prototypes for a particular reactor type ended up with almost all of the private industry orders for that type. Westinghouse constructed Shippingport, then Yankee, along with all of the advanced reactors that followed this important second plant; all except five of the twenty-six private pressurized water reactors subsequently on order through March 1967 were on contract to this company.[28] The General Electric Company was responsible for

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[27] An alternative assumption is that the subjective probability of final success was twice as great for the PWR than for any other design type before the fact. This does not change the estimates: the costs (weighted by probabilities) averaged 3.2 times those forecast, rather than 3.5, so that it might have been possible to do two projects successfully for the forecast costs of 6.4 rather than 7.0 projects.

[28] The others were contracted in 1966 and 1967 by Babcock and Wilcox, the producers of the one other PWR prototype constructed in the early 1960's; the exception might well demonstrate the rule.

the construction of the Dresden and Big Rock Point prototypes that made up the first and second steps in the boiling water reactor project, and this company completed most of the remaining prototypes as well.[29] General Electric contracted for 20 of the 23 boiling water reactors on order through the first three months of 1967, a substantial lead in sales of this concept (marginally contested only by Combustion Engineering).

Such results are not surprising. The achievement of a reactor design goal results from construction of reactor systems -- from "learning by doing" often enough to gain control of the interaction of new components and to improve on these interactions. Success was measured in terms of the operating performance of a reactor costing less than $200 per kilowatt, and if there were no prototypes, then no measure on this standard could be given. Grades on the first prototypes built by a company were invariably lower than on second or third prototypes of other companies built at the same time.[30] Then the observed relation between π and E for the two light water reactors was not only a particular path to economic capital costs, but also the "learning curves" for two separate corporations. At the end of the two projects, each had a decided advantage for sales of one or the other reactor type because they could and had built enough reactors to control costs and system performance.

The last part of the lesson is that the existence of more corporations in the market implies more competition and thus a faster spread of the benefits of research to the consumer. This is not always the case; a larger number of

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[29] Allis Chalmers constructed the important Elk River Reactor, but left the industry in the later 1960's; Combustion Engineering entered only at the last stages of prototype construction with BONUS which is scheduled for completion in 1968.

[30] For example, the first Allis Chalmers BWR had capital costs $60 per kilowatt higher than the second General Electric BWR finished in the same year. At a later point, the lead had changed so that the second Allis Chalmers experiment cost $800 per kilowatt less than the first Combustion Engineering BWR experiment. (These indicate no more than that the number of previous experiments, as well as the size of the reactor and the difficulty of the experiment, might be a factor in determining the costs of construction for the prototype).

competitors is not synonymous with market behavior characterized as "competitive." Three companies, all producing turbine generators, operated an organization for price setting in the 1950's that had all of the effects of monopoly; three was not a large enough number for competitive behavior to result from the price-output policies of firms.[31] On the other hand, two independent firms in other industries can and have produced price-cost differences, price flexibility and lack of discrimination between buyers that can only be characterized as "competitive."[32] In the case of nuclear reactors, the pattern of output and price formation does seem that the greater the number of research projects, and thus the greater the number of firms, the more extensive the competition.

The sales of reactors to private and government utilities began in earnest in 1965, but for delivery in 1970 or thereafter.[33] General Electric agreed to construct 5 of the 9 for delivery to domestic firms in 1970, 6 of the 12 for 1971, but only 1 of the 12 for 1972; Westinghouse was responsible for 3 of the 9 for 1970, 3 of the 12 for 1971, but 7 of the 12 for 1972 (with the residual supplied by Babcock and Wilcox, or by Combustion Engineering). The first company altogether accounted for 12.5 thousand megawatts of capacity on order, the second for 11.7 thousand megawatts, of a total of 31.3 thousand; each had exactly 20 plants under contract as of the third quarter of 1967.

Two aspects of this order pattern stand out rather clearly. The market for new reactors has been shared in a remarkably even fashion so far, 39 per cent

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[31] Cf. Testimony of M. A. Adelman in OVEC vs. General Electric, Docket 62 Civil 695, P. 4900 Et Seq.

[32] Cf. James Mckie, Tin Cans and Tin Plate (Harvard, 1959), Ch. IX.

[33] Cf. A.E.C. Authorizing Legislation--1968 (Joint Committee on Atomic Energy, Washington, 1967) pp. 707-709.

for General Electric and 38 per cent for Westinghouse. Also, the sequence of orders is quite separated in time, with General Electric "filling up" first while Westinghouse has taken almost all of the orders for later delivery. Both might be considered the results of a market sharing agreement between these firms; but there is no evidence of such an agreement, nor is it likely to have been in effect given the surveillance of these companies by the Justice Department and the buyers after the "great electrical conspiracy" was revealed in the early 1960's. Both aspects of behavior are consistent, as well, with a contrary hypothesis -- that each company ignores the output behavior of the other in the "Cournot" manner.[34] Each producer regarding himself as the single seller of a differentiated reactor concept -- one unique in enough aspects of performance to make demand separate from that of other reactor types -- would treat the demands of utilities as dependent only on his quoted charges for construction of system. This would be a mistake, since the other companies' charges would in fact be almost as important, but the result of the mistake would be to estimate demand to be equal to total reactor demand minus the average sales of the other firm. The two companies following this output strategy would each make about the same number of sales as the other; most likely, when one company started selling slightly before the other, each would make his sales "in turn" with outputs fixed by limits on available capacity.[35]

The implications of Cournot output behavior extend beyond equality of shares to a particular relation between the level of prices and the number of firms. When there is one firm, Cournot pricing is monopoly pricing; this

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[34] After A. Cournot, Recherches sur les Principes Mathematiquis de la Theorie des Richesses (1838).

[35] Where capacity is fixed by the demand forecast, of course.

price might be termed P and output termed q. When there are two manufacturers, as a result of the entry of a second firm, the new firm considers q as fixed and chooses an output q* for himself; this additional output causes the first firm to cut back on his original production to q**, because q + q* reduces prices under that for q alone. The roles of each are taken by the other over again, until $q^{**} = q^*$ and $q^{**} + q^* = 4/3q$. The result of the entry of a second firm is an increase in market output, and consequently a reduction in profit margins to one half the previous so as to "pass through" the fruits of invention.[36]

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[36] The general argument is that the producer "i" maximizes net receipts NR with

$$NR_i = q_iP(Q) - C(q_i)$$

where q_i is that firm's output, P that firm's price (and all firms' uniform price, a function of total output Q) and $C(q_i)$ that firm's total costs. All q_j, $j \neq i$ are assumed to be fixed by firm i, after Cournot. Then for maximum NR_i, $\partial NR_i/\partial q_i = 0$ which requires

$$P(Q) + q_i \frac{\partial P}{\partial Q} \frac{\partial Q}{\partial q_i} - \frac{\partial C}{\partial q_i} = 0$$

for all firms i. If there are "n" such firms, so that there are "n" equations, then

$$nP + \Sigma q_i \frac{\partial P}{\partial Q} - n \frac{\partial C}{\partial q_i} = 0$$

(for $\partial Q/\partial q_i = 1$ and $\partial C/\partial q_i$ the same for all firms). Then the simple decision rule for the firm's profit margin is

$$(P - \frac{\partial c}{\partial q}) = - \frac{1}{n} \cdot \frac{P}{\epsilon}$$

where ϵ is the elasticity of market demand (equal to $[P(\frac{\Sigma q_i}{P} \frac{\partial P}{\partial Q})]^{-1}$ in the previous expression. When there is one firm,

$$P - \frac{\partial c}{\partial q} = \frac{-P}{\epsilon}$$

the monopoly profit margins equal $-P/\epsilon$. When the number of firms is two, then the profit margin is half that of the monopolist. When the number is so large as to be beyond count, the profit margin -- the price-marginal cost difference -- is zero. This is shown most simply and clearly in W. S. Vickrey, Microstatistics (Harcourt, 1964) pp. 337-338.

Does the Cournot model describe the price behavior of the two light water reactor producers? Price reduction took place after it was known that there would be two producers on equal terms, so that prices for two were less than for one. Certainly the General Electric price list was reduced by more than 20 per cent after the Westinghouse contracts in 1963 had shown that this other company was going to reach the $200/kilowatt target, and the reduction took capacity prices well below $200. But there were quite obvious cost reductions for the two producers from learning to build reactors that explain these price reductions just as well as does the Cournot argument. The only indication that prices are more similar to the two-producer Cournot case than to monopoly is in the broad estimates of price-cost differences for these companies.

Prices had settled, on the basis of a dollar of constant purchasing power, "to around a little over a hundred,"[37] with a low of $95/kilowatt and a high of $120/kilowatt for capacity in the 600 to 900 megawatt range. All indications point to demand elasticity in the range of -1.3 to a high of -1.5.[38] Marginal costs -- the change in a reactor producer's costs from adding an additional order to his production list -- are of course not publicly known outside of the plants of the two producers; but an "order-of-magnitude" calculation can be made of these costs by adding up engineers' estimates of components and materials costs for a 600 megawatt reactor.[39] This assumes that there are lower costs for the last contract than for all of the others, given that engineering design expenses and assembly costs are not included at all (implicitly, these activities were

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[37] Cf. Remarks of Chairman Holifield, AEC Authorizing Legislation, Fiscal Year 1967 (Washington, 1966), p. 659.

[38] The econometric estimates of values in this range are discussed in detail throughout Chapter 5.

[39] The engineers' estimates are from the monographs in Kaisor Engineers, Guide to Nuclear Power Cost Evaluation, Volume 3, Equipment Costs, (March 1963, TID-7025, Vol. 3). They hold for the early 1960's; but it is assumed that later reductions on materials costs are cancelled by general price inflation.

"free" to the last contractee because resources to provide them had already been invested. The calculation shows that marginal costs could not have been less than \$60 per kilowatt, and were most probably in the \$70 to \$80 range. Cournot pricing for two firms has price minus marginal costs $\{P - MC\}$ equal to price divided by twice the elasticity of demand $-P/2\epsilon$ (for number of firms "n" = 2) while the absence of any effect from the second firm results in $(P - MC) = -P/\epsilon$. The estimate $(100 - 70) \sim -100/2(-1.5)$ is in accordance with the two-firm Cournot description; price is \$100 per kilowatt too low, or marginal costs more than \$30 too high, for the second firm not to have affected the level of output and prices.

Summary

There is little more evidence to firmly establish either lesson. But, rather than maxims for all future decision making, these are indications that money was probably spared by starting more parallel projects and that a second successful project reduced profit margins. The question is whether future large scale research projects will not repeat this history only because it was not taken into account.

3. STRATEGIES FOR DEVELOPMENT OF THE FAST BREEDER REACTOR

The federal program for developing thermal reactors seems to have passed important landmarks in the last two years: those engaged in the research are no longer the only ones making forecasts of reactor use, but rather have been joined by those using the equipment; those purchasing equipment have done so without capital subsidies from the Federal government. The transition from hypothetical to real demand for reactor capacity marks the end of thermal research as now conceived, and this research would only be started up once again if the forecasts of the buyers proved to be grossly inaccurate in matters of efficiency and comparative costs. But this is not the end of reactor research. Most of the questions of research design and, ultimately, research strategy that came up in the thermal reactor program are now being asked of schemes for constructing and operating fast breeder reactors in the 1980's and 1990's.

The fast breeder reactor promises a set of research problems of the same order of magnitude as those encountered in the thermal reactor projects. This new type of reactor also promises to produce both energy and plutonium from fission -- to exceed the output capabilities of existing reactor systems by producing its own fuel. This promise for increased outputs is contingent on fitting the new system into the ongoing demand for thermal reactor so as to provide net social benefits from additional research expenditures. But this is a matter of both forecasts and strategy -- forecasts of the effects of this additional source upon supply and prices of nuclear capacity, and strategy to decide whether or not the project should be continued at various turning points in its lifetime. The methodologies of forecasting, and the strategies, are derivative from the general economics of project evaluation.

The Concept of a Fast Breeder Reactor

A fast core consisting of uranium, plutonium, and coolant encased in various metals seems less complicated than that in a thermal reactor because it does not contain a moderator; but it has much more complex behavioral patterns. The neutrons released in fission possess high kinetic energy and retain this energy in the absence of moderation when scattering through the core. Those colliding with natural uranium (U^{238}) cause fission at a higher rate than when a moderator is present, and also cause a higher rate of non-fission reaction. The series of events in this last case consists of the conversion of U^{238} to U^{239} and ultimately to plutonium Pu^{239} with fissionable nuclei in relative abundance.[1] But the production of both heat energy and plutonium in fast cores varies widely from one second to the next because the number of productive collisions changes extremely quickly. The reactivity ρ -- the percentage rate of birth of neutrons in excess of those holding the population constant -- changes with coolant temperature and volume, and with the size and shape of the core. The changes in temperature, et.al., in part stochastic and in the other part interactions of dynamic nuclear equations, are of an order of magnitude greater in a fast reactor so that there are relatively large variations in reactivity whenever the reactor is operating. These variations take place more quickly and with higher energy levels in a fast reactor and they render the reactor more prone to accident.

The fast reactor contains all of the usual subsystems -- fuel rods assembled into a core, heat exhangers, turbine generator leading to a condensor.

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[1] When plutonium produced exceeds the volume originally inserted in the core, then the apparatus is said to "breed."

There are differences in standards for successful operation, however. The core is extremely compact because of the absence of a moderator and to achieve the higher power densities (kw/g of fuel) that make up for the burden of its high plutonium content. The heat absorption capabilities of the coolant are of the highest order for the same reason. Setting such standards has ramifications elsewhere in the system, however: high power densities and coolant pressures lead to extreme temperatures in the core in the event of accidental breaks in the system; the security of tubing and structures has to be increased to minimize such occurrences. Also, the fuel element assembly is especially difficult to construct -- the small diameter fuel rods, and the large temperature changes which they experience, require more of materials and design than now achieved in the construction of thermal fuel cores.[2]

These requirements are accentuated by the suspected behavior of fast systems under abnormal conditions. Not enough is known to allow strong statements, but in the absence of a moderator the level of reactivity seemingly depends only on the compactness of the fuel core. If some event increases the fuel/coolant ratio per unit volume, then there will be an accompanying increase in reactivity which could push the heat energy from fission beyond that tolerable for reactor integrity (in contrast, thermal reactor cores are held within prescribed temperature limits by expansion of the moderator and its relative dampening of reactivity). Whatever the untoward event, it not only occurs in a compact environment with higher temperatures and pressures but also is much faster: the life span of a neutron in a fast core is 1/100

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[2] Cf. T. J. Thompson and J. G. Beckerley, The Technology of Nuclear Reactor Safety, Vol. 1 (M.I.T. Press: Cambridge, 1964), p. 158.

that in a thermal core, and energy is exponential with respect to neutron births per unit time. Once a fission-induced power surge begins, further increases in fission take place much more rapidly in a fast breeder where neutrons multiply rapidly and profusely; detection and interruption of this process -- the essence of control -- remains to be accomplished with the certainty now experienced in thermal reactors.

The fast breeder, difficult to construct, is supposed to operate at the extremes of materials tolerability in a fashion difficult to control. The surprise is that there are at least three candidate systems.[3] The Liquid Metal Fast Breeder uses liquid sodium to take heat from the core through multiple heat exchangers to steam turbine generators. There are advantages in doing so -- sodium has excellent heat absorption capabilities from 200°F to 1600°F at atmospheric pressure -- but this metal welds joints and bearings, explodes on contact with air and water, and is so absorptive that thermal shock is transferred quickly from some spot in the core to all parts of the system. The Steam Cooled Fast Breeder passes low temperature steam from a simple boiler through the fuel core to produce high temperature steam which goes directly to the turbine generator. This simple system avoids heat exchangers and any special enclosures to isolate the coolant, but it delivers radioactive steam to the turbine generator. The closed cycle also raises problems of maintaining steam under pressure in the core if there is a break elsewhere. The Gas Cooled Fast Breeder combines the strong and weak aspects of the other candidates. Helium is transmitted through the core under high

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[3] There are a host of further candidates other than the these discussed here. But these three are blessed with designs for 1000 MW_e systems, working mock-ups, experimental components -- even working prototypes, in one case.

pressure to a set of heat exchangers where steam is then generated. The safety of this coolant matches that of steam, but the high pressure of the system is as much a disadvantage in both. The compartmentilization of a gas fast breeder is similar to that in the liquid sodium breeder, as is the complexity of heat exchanger mechanisms.

Designs have been completed of reactors in each of these three systems for producing electric power for less than four mills per kilowatt hour. In the case of the liquid metal system, prototypes have been constructed in the United States (the Enrico Fermi Plant), Great Britain (Dounreay), and the U.S.S.R. (the BN series). But these reactors have not provided continuous power capacity to commercial standards. A meltdown of a core section of the Enrico Fermi plant after intolerable core temperatures rendered that plant inoperable through a good part of 1967, for example; the others have been carefully operated to carry out controlled irradiation of fuel rods, and incidentally to produce steam. There are no comparable prototypes for gas and steam cooled systems. The fast reactor remains to be developed.

Forecast Results from Successful Introduction of Fast Breeder Reactors

Problems to be encountered before the construction of the first demonstration fast breeder reactor seem formidable. But they are not impossible -- there is some expenditure on research and experiments in small scale systems that will produce preliminary results making it possible to proceed with the large scale demonstration plant, just as there were sufficient expenditures to bring about such projects in thermal (light water) nuclear reactors. This amount of money may be exceedingly large, but the research results meriting construction of a demonstration plant can be purchased.

These costs of research cannot only be undertaken, but they are amenable to forecasting. Any number of forecasts is possible and at least three forecasting techniques come to mind that provide different estimates of total expenditures from the initiation of a program in the early 1970's to termination of the research portion of the large scale demonstration reactor in the middle 1980's. The first method is to extrapolate the behavior in the light water program: using the curve for the functional relation between research results and outlay for the two successful light water programs, the additions to outlay to complete the program can be forecast by observing the multiple of present expenditures required to move from the present results to terminal full success. The second technique would be to ask a sample of knowledgeable nuclear engineers what the costs of a fast breeder program would be if the program were given specific targets to achieve by 1985. The third technique would be to carry out economic and engineering research on the nature and extent of the technical problems in each subsystem of the breeder reactor project and then set a "price tag" for the solution to each of them. The sum of these price tags would equal the total costs of one or the other of the specific fast breeder reactor projects.

The estimates of costs from these three calculation procedures could well differ by substantial amounts. Before the fact, the best procedure might well be a combination of the last two, however, and the calculations obtained therefrom might be given greater weight. The first would have to be based on very preliminary results from past fast breeder research; the combination would rely centrally on the opinions of experts as to the nature of problems and the cost of solving them and thus would be directed more to the

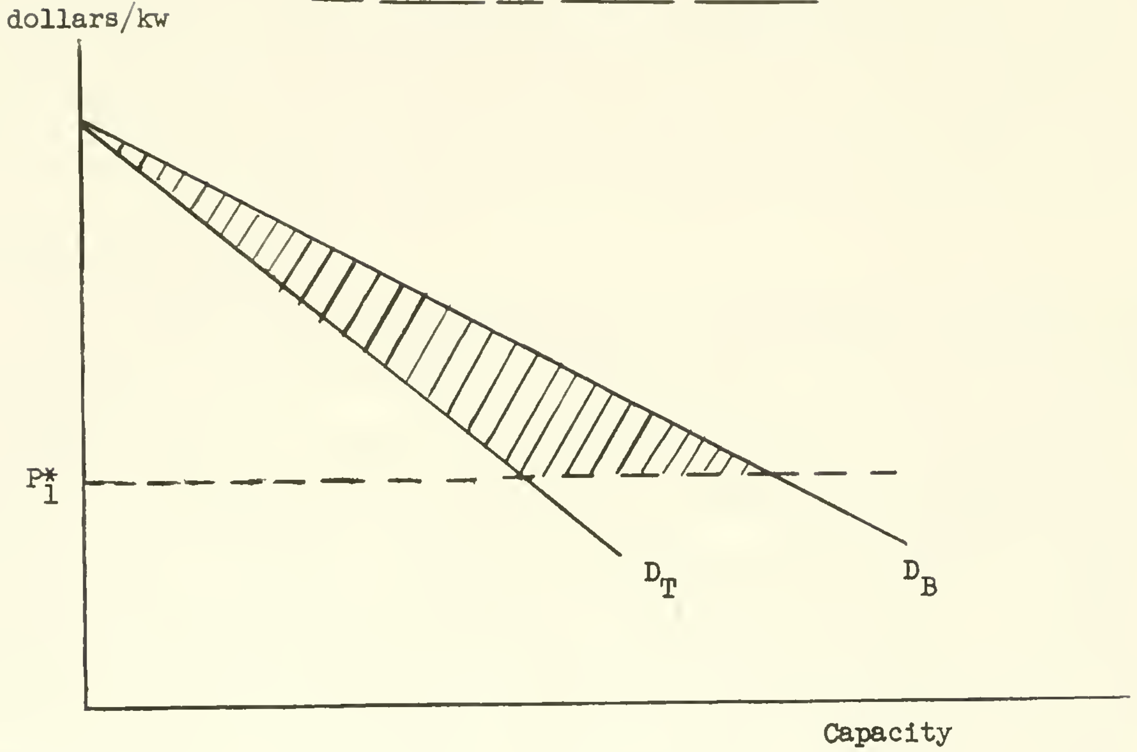
future (about which more is known, from the design studies and from engineering literature directed to problem solving).

Balanced against the cost of research would be the substantial economic effects from the introduction of successful breeders. After the demonstration plants have shown that the breeder will produce thermal energy for sustained periods, then electricity generating companies will seek contracts for the construction of these breeder plants. The contracts will be signed by the companies and one of the manufacturers of nuclear equipment; the transactions will be for capacity to produce thermal and electrical megawatts in plants with designated load factors. Their number will depend upon the prices (per kilowatt) at which breeders are offered, and their expected fuel cycle costs as compared to thermal nuclear plants and -- basically -- fossil fuel electric generating facilities. The latter cost should favor large numbers. The breeder promises lower fuel cycle costs to the equipment manufacturer and ultimately to the electric generating company, because it produces fuel either for sale in the market for nuclear materials or for re-use in the next round of energy production in the plant doing the breeding.

The price is a matter of strategy of the equipment manufacturers in the market for breeder and non-breeder reactors. As long as the special advantages of the breeder are considered by the manufacturer in setting the price, however, the breeder will have its own demand. This is shown in Figure 4, where D_T is the demand for thermal reactor capacity in some properly defined regional market as a function of dollars/kilowatt of installed capacity. This is historical and institutional demand, but it also depends on the fuel cycle costs and the costs of alternative sources of supply of

Figure 4

The Demand for Breeder Capacity



generating capacity such as provided by fossil fuel equipment manufacturers. The demand for breeders D_B is greater, given the same conditions as for D_T but that fuel cycle costs are lower, since these costs are expenditures on a complementary input factor to accompany capacity (the reduction in price of a complementary good always increases the demand for the good in question). Fuel cycle costs are determined in the markets for fuel and by the efficiency of the breeder core as a fuel generating device; since it is difficult to conceive of the equipment manufacturer collecting a "toll" on the fuel, it is most likely that the fuel cycle costs will be passed through the manu-

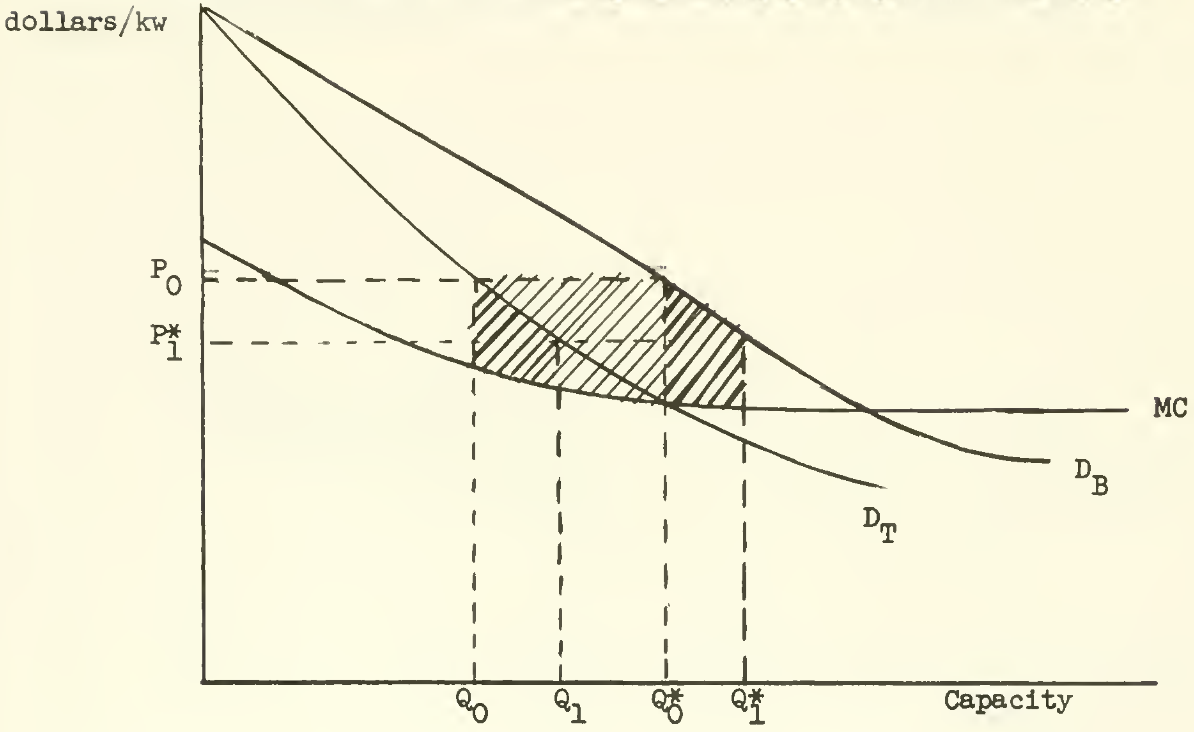
facturer to the user at a level reflecting all of the cost savings in breeding. Then the difference between thermal demand D_T and breeder demand D_B might well be substantial and the effects of these differences on consumer demand -- the consumers' surplus shown in Figure 4 as a cross-hatched area -- will also be substantial.

The introduction of the breeder reactor should reduce the charges made per kilowatt of installed capacity for all reactors. The history of pricing of thermal reactors is marked by adherence to the results of the Cournot model; if this history is continued into the future by new firms producing this new type of reactor, then the results should be the same as if there were more firms added to the market for nuclear power plants. The price-marginal cost difference should be reduced in inverse linear relation to the number of firms. The direction, if not the extent, of such price changes from independent entry resulting from production of the breeder is shown in Figure 5. As price is reduced from P_0 to P_1^*, installed capacity of thermal reactors is increased from Q_0 to Q_1 with a consequent increase in consumers' surplus as shown by the cross-hatched area between the demand curve D_T and the marginal cost of installed capacity MC. This price reduction increases consumers' surplus from the breeder reactor as well, as capacity under order or under construction increases from Q_0^* to Q_1^* and the surplus is enlarged by the cross-hatched area shown between the demand curve D_B and marginal capacity cost curve MC.

The costs of capacity may also be substantially decreased by the introduction of breeders. After research is complete, the results may well exceed

Figure 5

The Price Effects from the Introduction of Breeder Reactors

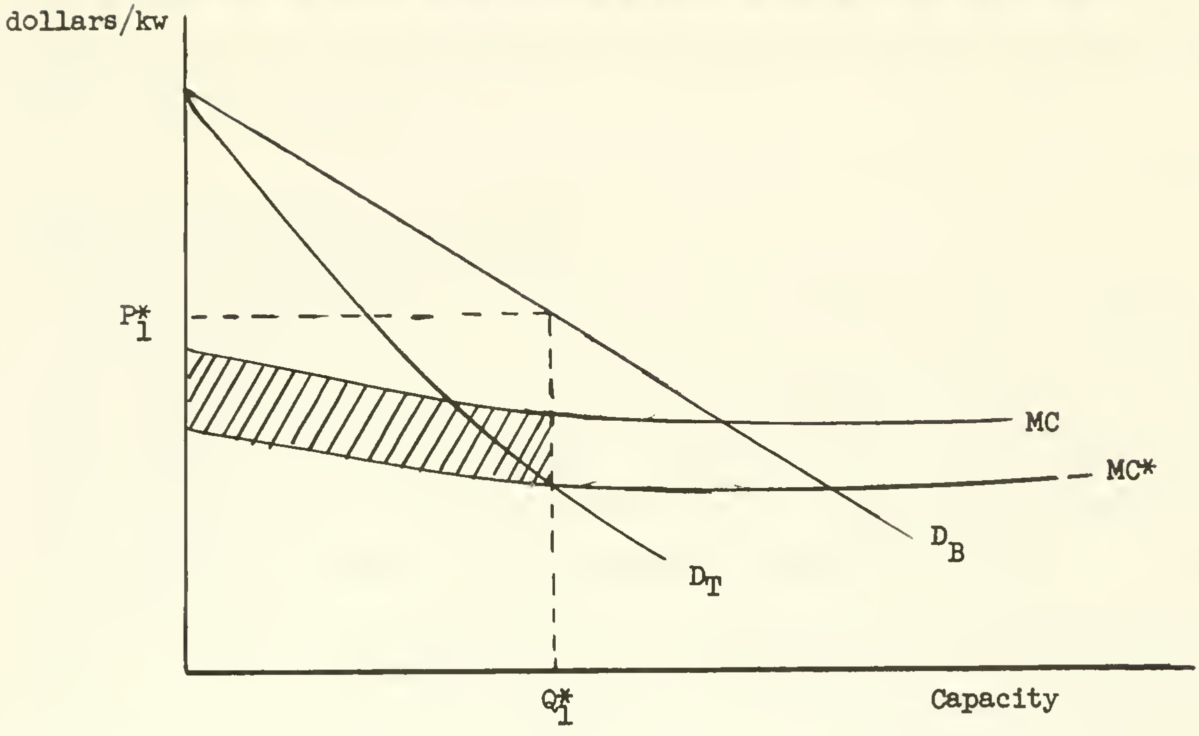


forecasts for the performance of the nuclear reactor construction companies, and the marginal costs of installed capacity may be much less for breeders than now being forecast for thermal reactors. This possibility is shown in Figure 6: the marginal cost curve MC for thermal reactors is undercut by the costs MC* for the construction and installation of nuclear fast breeder capacity. Then the gains to the economy are shown by the cross-hatched area between the two curves for the amount of installed capacity Q_1^* in keeping with the charge P_1^* following the introduction of this new type of reactor.

The forecast is for substantial economic gains from the breeder, from three sources. The first is consumers' surplus generated by increased demand from lower fuel cycle costs, and the second is surplus from the price

Figure 6

The Cost Effects from the Introduction of Breeder Reactors



reductions resulting from the separate and independent entry of new firms producing the new reactor. The third is pure cost savings -- less resources required to construct new reactor capacity per unit of capacity -- from this innovation.

All three sources are quite tentative. The surpluses may never appear, because the specific characteristics of the breeder inhibit demand so that the differential between D_B and D_T in Figure 4 does not appear (for example, as a result of utility companies inhibitions against new reactors when the more established types are operating consistently). Prices may not be

reduced because the firms producing breeders are those already supplying thermal reactors. There may be no cost reductions in capacity construction at all; certainly it is difficult to find credible forecasts for such reductions at the present time.

General Considerations of Strategy

Even though forecasts of the effects of breeders on the economy are most tentative, there is general agreement that they could well turn out to be very large. The demand forecasts show that more than 800,000 MW of nuclear capacity will be installed between 1980 and 2000. If the savings in fuel cycle costs generate only the smallest conceivable surplus -- an amount equal to itself -- then each reduction of fuel costs of one-half mill per kilowatt hour with that amount of capacity would generate a $3 billion surplus. A price reduction on all new capacity whether thermal or breeder of $25 per kilowatt would be modest in Cournot terms, but would generate $20 billion of market-wide price reduction and (at least) one-fifth this much consumers' surplus from additional demand for capacity. Cost savings of $5 per kilowatt -- which are not outside the realm of present forecasts -- would generate another $4 billion of surplus for the economy. There is little basis for arguing that the gains after 1980 could not be an order of magnitude greater than $1 billion of expenditure on research before 1980. The question is whether these social gains are enough greater to justify a single large scale project to develop a fast breeder reactor.

The choice of whether or not to carry out research and then develop a fast breeder reactor is in good part a matter of more detailed forecasts of

economic effects from such a project. The Liquid Metal Fast Breeder Reactor project promises specific but uncertain outlays each year from (for example) 1970 to 1985; the present value of these outlays is $\sum_{i=1}^{15} C_i/(1+r)^i$ where C stands for expected expenditure and r for an appropriate rate of discount of this expenditure. This project promises results, as well, equal to $\sum_{j=15}^{n} R_j/(1+r)^j$ in dollars equivalent to the gains shown from the shaded areas in Figures 4-6 in each of those years in which new capacity is installed. The rate of return r which makes equal the first fifteen years of outlay and the succeeding years of economic gains may or may not be large enough to justify taking on the project.

The Liquid Metal project can very well produce more than a single set of research results, and more than one response by utility companies to these research results. There is a finite probability that research outlays will exceed C_i in any year and that the gains obtained therefrom will fall short of R_j in succeeding years. The rate of return that makes a research outlay equal to the subsequent benefits in present value terms would be r^* rather than r. Each probable schedule of research expenditures, and of reactions of demanders to the results of the LMFBR demonstration program, generates a new value of the internal rate of return. This is the case for the steam cooled breeder and the gas cooled breeder programs, as well: most probable research outlays and buyer's reactions generate values for internal rates of return, and less probable results generate a succession of lower and higher values. These can be arrayed for each type of breeder separately

as frequency distributions of expected rates of return so that, as a beginning of determination of strategy, there are three frequency distributions of r for the three most likely breeder research projects.

The economics of strategy calls for a comparison of these distributions. If one of them has both the highest expected value for r, and the lowest dispersion from this value, then this is the preferred project.[4] The question is then whether the expected value is higher than for alternative research and development projects with comparable dispersion, where these alternatives are competing for government funds or are in the private sector competing for the research funds allotted by companies. If the rates of return on the most favored breeder project are higher than these alternative returns on research, then this single project should be undertaken at least at the initial stages.

The choice of the favored project, and of this project over alternative research in other industries, is more difficult when the higher expected value of the internal rate of return r is accompanied by more variation in this rate than found on other projects. Then the choice is between a project with a higher expected return but more risk (as measured by the variance σ_r^2, for example) and a second project with lower average return and less risk. But even here the choice involves going to the market place for alternative cost: if the average rate of return accompanying such variance on projects in research industries is less than or equal to that in this breeder project,

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[4] This is the optimal strategy when the consumer's gains are measured in terms of the utility of dollar returns, and the utility function is of a particular form. The requirement is that utility decline at the margin as returns increase -- that a loss in returns always has more significance than a gain -- so that total utility $U = f(r, \sigma_r^2)$ and $\partial U/\partial r > 0$, $\partial U/\partial \sigma_r^2 < 0$. Then the choice of the project with smaller σ_r^2 is obvious.

then this project should be undertaken. This is because the alternative costs of this research are at most the returns that could be made on research elsewhere subject to equal risk.

Economics of strategy does not end with choice of a single project, because it may be possible to show that the addition of a second project also provides net returns greater than alternative costs for that second project. The successful completion of a Steam Cooled Breeder Reactor program, for example, would add to research expenditures beyond those called for in a Liquid Metal program (if the Liquid Metal program were chosen first, for example). The Steam Cooled Breeder would not add the cross-hatched area shown in Figure 4 to compensate for these costs, because the first-favored program would have already accounted for these gains (although lower fuel cycle costs for a second program might shift D_B farther upwards and to the right than shown, and the additional cross-hatched area resulting from the shift would have to be credited to the second program). But there should be price reductions as a result of the added program because there should be new firms producing nuclear reactors; the cross-hatched areas shown in Figure 6 should be credited to the new program, but with P_0 equal to the price of reactor capacity when there is one breeder and P_1^* equal to the price schedule after the second breeder has been introduced. There could well be cost savings from the Steam Cooled system as compared to the Liquid Metal system as shown by the cross-hatched area in Figure 6 (or as shown by some percentage of this cross-hatched area for whatever part of the market adopts this last type of breeder). The rate of return which equates these economic gains with the initial outlays for Steam Cooled Reactor research may be

greater than the return from alternative projects. If the uncertainty attached to such returns -- as shown by the variance in the frequency distribution of r for the steam cooled breeder -- may also be less than in alternative projects, so that it pays to take on this second breeder project.

The best of all projects could be a combination of three endeavors to build breeder reactors. A combined research program in first solving physics problems in control of fission in the core, and then constructing experimental systems for taking the heat from the core by means of different transportation media, can cost less than the sum of separate liquid metal, steam, and gas projects. This is because the consolidated project allows more work to be done in series with less overlap of research, in separate liquid metal, gas, and steam programs when the research involves exactly the same procedure for solving the same problem. All of the benefits of substantial price reductions from the addition of second, third, and fourth firms can still be captured by having independent firms bear the responsibility of carrying out the systems and design research subsequent to developing the fuel core. Separate companies with separate breeder types ought to produce the maximum possible increase in the net value of output shown in Figures 4 and 5, and may result in substantial cost reductions as shown in Figure 6. The rate of return expected from a consolidated project, and deviations from this rate of return as a consequence of deviations of performance, can be calculated. If it seems that more than one project is called for, then it is very likely that a consolidated project would offer a higher expected rate of return, and no increase in variance in rates of return.

This is schematic economic strategy to be introduced at the first stages of project evaluation. As data are collected on research performance,

the same schematic applies later, however. Further information on the cost of research of various projects may change the rankings of projects by changing the expected values and variances of r. Every time that new information is sufficient to allow recalculation, then the strategy should be reapplied: choose the project with the most favorable distribution of rates of return, and then add projects if their distributions of returns outclass those of alternative research programs in other industries. Finally, choose a consolidated project made up of separate breeder programs (with research merged where it is identical in the separate programs) if the distribution of r for this project outranks that of the first or most favorable separate project.

4. THE COSTS OF FAST BREEDER RESEARCH AND DEVELOPMENT PROGRAMS

Research groups in the national laboratories and private corporations working on the development of fast breeder reactors all have the same loosely defined goal -- to produce a breeder design that convinces electric companies to construct these reactors because capital and fuel per kilowatt-hour of power are cheaper than in other systems. The paths to this goal diverge for the gas coolant, the steam coolant, and the liquid metal research groups, but the general techniques for achieving it are quite similar. Each group does research in basic physics and develops components for a "first-of-a-kind" system that can generate steam or electricity. Improvements in the components and in the routing systems of this prototype are built into a second stage reactor; costs of power are measured at this stage, and, if there is promise of greater reductions, a third round will be constructed as a large scale demonstration reactor. The demonstration reactor is to do what its name suggests -- show the firms likely to contract for reactors that a particular fast breeder type can produce with consistency and without overwhelming fuel cost disadvantages.

The technical hurdles which must be overcome to reach this objective are formidable. Physics research must be completed at the most basic levels before the development of fuel elements capable of burn-ups beyond those presently experienced. Applied research towards more durable materials to be used under a wide variety of operating conditions -- including conditions different from those posited by the reactor design -- has to be done in laboratories with small-scale models of fission processes, or in experimental reactors. Components have to be developed, for example for the coolant system, in place of high cost experimental components (and these new pieces of equipment have to be made operable in the reactor). Last and perhaps most difficult of all,

reactors have to be built that operate. The whole is greater than the parts put together, since successful integration of all parts of the apparatus into a workable system requires control of both normal and abnormal interactions of the components. In the history of reactors, the development of low cost components proved to be only a small item in the task of putting together a workable system; there seems little reason to believe that this will not be the case in the even more complex systems required for fast breeding.

Solutions in each of these problem areas require extensive research and development expenditures. The review of the program that follows is for the purpose of forecasting these expenditures, to show their expected levels for a normal development path and also these levels either given research "breakthroughs" or research "setbacks." These three levels of costs are polar cases, because two at least reflect the extremes of the probable results; but they are also more than just contrasting cases, given that research results occur within each of three categories of events. The first category is made up of windfalls -- easy or inexpensive solutions to major problems of materials development, mostly from work being done in other fields. The assumption is that only the most probable breakthroughs occur when the "low estimate" of costs is calculated. The second category includes all those occurrences required to keep research and development on the time and cost schedule now laid out for any particular project for the period 1965-85. Costs of research in this category have been estimated by the corporations and national laboratories (in some cases, extrapolated beyond the present five- or ten-year planning period) so as to obtain the funds from the Atomic Energy Commission to carry out the projects, where projects have been proposed; otherwise, costs have been estimated here

as those to be incurred on the research path proposed, and then tested against the reactions of a number of corporate research managers for comparability with what they would submit. The third category is made up of events that can only be called serious failures in development of important components; not all parts of a project are assumed to fail, but those viewed with the most suspicion are treated as if the fears were well founded. Costs in this case are those from projects which finally reach the design goal, but only after substantial extensions of program inputs beyond those forecast; costs are higher than "forecast" (the costs from results in the second category are "forecast") because of relatively poor results at each stage -- similar to results realized in the ten-year program for developing thermal reactors.

The estimates are for those events either widely expected or widely feared, given the evaluations of the fast breeder appearing in the nuclear engineering literature. But they are all constructed, or put together here from widely scattered sources, for the first time. They stand only on the judgment of the author and on the procedures by which they were made or collected.

The Liquid Metal Fast Breeder Reactor: Fuel Development

Reactor designers are fond of saying "provide me with a fuel element and I'll build you an efficient reactor." To produce such a fuel source, the Materials Test Reactor (MTR) was designed and then constructed, early in the 1950's, as the first step toward the economic thermal reactor. The first step towards a comparable liquid metal fast reactor is a fuel rod that lasts longer and makes possible more complete burn-up of the plutonium than is now being experienced.

Fuel cycle costs are extremely sensitive both to the extent of the "burn-up" of the original inventory of plutonium, and to the ability to produce

thermal energy from fission of a gram of this inventory. The target burnup for LMFBR fuel has been set at 100,000 MWD/T; if only 50 per cent of this rating is achieved, .5 mills/kwh are added to the fuel cycle cost. If the realized linear heat rates -- kilowatts per foot of installed Pu -- are only 50 per cent of the 1963 design study target, the added fuel cost is .33 mills/kwh. Hence there are compelling economic incentives for achieving target performance, which in this case requires both the 100,000 MWD/T and linear heat rates between 30 and 40 KW_e/ft.

Attainment of this burnup requires considerable extrapolation of present fuel element performance. Normal burnup in present light water reactors is close to 20,000 MWD/T, with deviations dependent on the linear heat rates (KW/ft of fuel) differing from roughly 16 KW/ft. This level of performance has been consistent with coolant outlet temperatures to date of about 1050°F. In the LMFBR, temperatures of 1250-1400°F are considered necessary to achieve linear heat rates of 30-40 KW/ft, and burnup of 100,000 MWD/T of plutonium.

At the present time, only 12 mixed oxide fuel rods at 15-16 KW/ft have been irradiated in EBR-II to 50,000 MWD/tonne. These tests are being extended since it is obvious that much more irradiation will be required before the specified fuel elements can be said to be "developed." Only EBR-II is available for more irradiation tests although FERMI may become available.[1] But EBR-II has 15 ± 1 core subassembly positions, each of which holds 19 encapsulated fuel rods or 37 unencapsulated rods. Most of the fuel rods irradiated in EBR-II have been encapsulated -- have had an added outer jacket of stainless

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[1] Dounreay, the Scottish reactor, is also a possibility although only two core and two blanket positions are available so that its usefulness is marginal.

steel for added safety -- and thus have experienced lower burnup than the subassembly would likely achieve in an operating reactor. In order to enlarge the testing capability and improve the forecast accuracy of the results, consideration is being given to using unencapsulated rods. There would be twice the number which could be irradiated at one time, and they would eliminate the reactivity penalty. But unencapsulated rods involve increased risk of greater contaimination of the system in the event of fuel element failure. Since failure of a fuel subassembly has led to the current shutdown of the Fermi fast reaction, in Detroit, this risk is not trivial or unimportant.

A complete research and development program for new fuel elements is expected to require approximately six years, including three years of irradiation in an EBR-type test facility.[2] The program requires several experiments, a number of which are in sequence. The first is to demonstrate the stability of fission under irradiation over the entire lifetime of the core. For burnups of 100,000 MWD/T, the experiments would take roughly four years in EBR-II; there are plans, however, to build a new, high-temperature, high-capacity test facility -- the Fast Flux Test Facility (FFTF) -- which can reduce the time span to a single year. The fuel rod must be subject to transient power bursts, as a second experiment, to verify that it can take sudden reactivity changes without melting or changing shape so badly as to disrupt the flow of coolant through

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[2] Current plans call for initiating a prototype in 1969 well before this fuel program is complete, so that first cores in near term prototypes will have conservative designs and low burnups.

that part of the core.[3] The AEC has indicated that a Safety Test Facility (STF) may be required for this purpose. A third round of experiments has to be conducted on fuel performance under extreme conditions; in a closed loop of the Fast Flux Test Facility, defects should be created in the cladding to observe changes in coolant contamination and in the radiated power of the fuel. Finally, a round of experimentation has to be completed to document the mechanical properties of fuel and clad after irradiation. Properties such as ductility and cycle strain undergo significant degradation after burnup is partially complete, and it is suspected that the extent of quality reduction is greater under the high-temperature conditions posited for fast fuels. In addition, metallurgical changes take place in the fuel as the many fuel element materials interact.[4] Observation is called for of those phenomena under a variety of burnup conditions. Variety requires the irradiation of a large number of fuel pins so as to add to expenditures on this research effort to develop economic fuel elements.

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[3] Solid fission fragments can cause from 20 to 30 per cent expansion of the fuel during the irradiation process. Much of this expansion can be contained by making the effective fuel density of compacted plutonium oxide only 75 per cent of the maximum possible density. This allows room for expansion, although it reduces the reactivity of the fuel pin and the thermal conductivity of the fuel. Fuel swelling is reduced at high temperatures (1000-1800°F) in oxide fuels since nearly all fission gases are released as the target for burnup is approached.

[4] The physical properties of fuel and clad undergo changes in the reactor as a result of both irradiation and interaction with the coolant. Sodium bonds, for instance, facilitate heat mass transfer from fuel to clad, but sodium itself forms a ferritic surface layer on austenitic stainless steel clad and removes soldering compounds so that self-welds are formed at various joints in the system. It is necessary to measure the thickness of the ferritic layer to assess the importance of corrosion under various conditions. In addition, changes in creep rupture strength, steady state creep rates and fatigue strength must be measured for a variety of sodium temperatures and pressures and for different levels of irradiation.

The time sequence for these experiments is unclear. Some part of the results will have to be accumulated by 1974-1975 from use of EBR-II.[5] The data will be limited mainly to burnup studies of oxide fuels, so that "best results" will point to development of the first oxide fuel element at that time. This is not the end: predicted performance of carbide fuels is much closer to the target requirements, so that work on carbides should intensify after 1975.[6] There will have to be continuous use of the FFTF to develop fuel elements of carbides and allows, as well as to extend the tests on oxides. In particular, the availability of FFTF closed loops after 1974 is necessary to carry out the destructive testing of fuel rods as soon as possible after the carbides and the newer alloys have been shown to meet some of the irradiation requirements.

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[5] The following table indicates the present status of fuel testing in EBR-II:

Number of Rods Removed from EBR-II

	Prior to 1967	In 1967
Mixed oxides	8	61
Mixed carbides	6	3
Alloy	35	5
Structural materials	500	900

The schedule of future EBR-II irradiations, in the Argonne Laboratory Study ANS 101, shows six subassemblies exposed to 100,000 MWD/T in the next three years, with over twelve additional subassemblies irradiated to levels of 50,000 MWD/T. These plans for this particular facility demonstrate a concentration on near term developments sufficient for design of oxide core fuel elements for the first large prototype.

[6] Carbide fuel is of interest because of its higher density and thermal conductivity, but there is a major difficulty in this material caused by mass transfer of carbon by the sodium to the clad so as to reduce the structural stength of the clad. The most promising fuel is U-Pu-Zr alloy, which has a higher melting point than uranium alone (1150° to 1180°C versus 1130° for U), is more compatible with the stainless steel (used in the heat transfer system), and has a very high thermal conductivity. Studies have shown that by reducing the fuel density to 75 per cent, up to 30 per cent volume expansion of the fuel is permissible without straining the clad. But these studies have also shown that the penetration rate of impurities into the clad has been temperature dependent, and burnups have been limited to the 4.9 per cent achieved in EBR-II.

The course of the fuel development program then is extended by the Fast Flux Test Facility, which, according to Atomic Energy Commission prediction, will become available in 1974. After some three to five years of exposure to FFTF test conditions, enough information should be available to develop the first carbide fast fuel element, and the second oxide element. Further development will require the testing of elements in prototypes after preliminary exposure in the experimental reactors; also, it appears that several years experience in a prototype will be essential before conclusions can be reached. If the first prototype is not operational until 1977, then 1980 would be the earliest time for completion of a second, or improved, carbide fuel element; if available earlier, as a result of great success in putting together a medium-sized prototype, then this testing would have results in 1978. (Several other prototypes could follow within a year or two of the first, from this optimistic view, so that there would be considerable operating experience with several fuel alloys by 1980.) Either the optimistic or "realistic" view suggests that second round fuel elements will be available in 1980, then. In fact, this is an easy achievement for a 50,000 MWD/T element, or for a 100,000 MWD/T element with quick success in prototype construction, or a difficult target for development of a 100,000 MWD/T element given abnormal delays and some important components failures.

The total costs of this 1967-1980 program in fuel research can be estimated. The Atomic Energy Commission's forecasts for expenditures for projects in the next five years are used as the basis for this estimate. First, the construction costs of the FFTF are taken to be $87.5 million, the amount of

the authorization requested of Congress and the Bureau of the Budget by the Commission.[7] Next, the FFTF is expected to cost \$15 million each year after it reaches operational status. This amount is scaled up from operating expenses of EBR-II, since the EBR is a much smaller reactor (62.5 MW_t as compared to 400 MW_t for the FFTF) which operates at lower flux levels and has no closed loops. It seems apparent that most phases of the FFTF operation will be more complex -- for example, documentation of the core physics of experiments in the loops is more difficult and time consuming because of more diverse effects from the higher flux levels -- and the costs of EBR-II have to be increased in any estimate for the FFTF to reflect such complexity.

In contrast, the actual costs incurred in the operation of the EBR-II are expected to diminish after the FFTF becomes available. This smaller test facility will be replaced by the FFTF for the major irradiation tasks but will continue in operation, or at least be maintained in operating condition, since otherwise there would be no backup in the event of an FFTF failure. Its importance should decline given that it will probably be used for long term irradiations of fairly well analyzed fuel subassemblies rather than for development and testing of new fuel elements. Hence it is assumed that the average operating cost of EBR-II will not exceed a low level of \$7 million per year.

Finally, the costs of general materials research for fuel development are expected to attain the level of \$20 million per year. Included in these costs are those for development of three to four different types of fuel

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[7] In previous testimony, Mr. Milton Shaw of the AEC had estimated the cost of the FFTF to be \$75 ± 15 million. Difficulties in construction and operation of this large fast reactor should result in costs reaching the upper limits of the estimate -- as in the case of every other fast reactor, from Fermi in the United States to BR-5 in Russia.

materials, for metallurgical testing outside of a reactor, for thermal flux exposures of new fuel elements, and for safety tests of new fuel assemblies. When the FFTF and several prototypes are in the construction stage, these costs are assumed to rise so as to prepare new materials for in-reactor testing.

The costs of all of these steps together can vary because of changes in the time schedule and magnitude of the results. If all parts of the project are on schedule, then costs are estimated to be as shown in Table 3. This amount of $644 million assumes that the FFTF is built to schedule, is operated to full capacity for four years, and is complemented by full operation of EBR-II when problems arise. As a matter of course, the problems may come at such a pace and of such a magnitude that excess capacity in the EBR is not sufficient to stay on schedule. Expenses associated with the FFTF could then be easily as much as 50 per cent greater than expected, adding about $40 million to total costs. Similarly, it can well take much longer to develop an adequate fuel for the fuel rod -- as much as five years longer, so as to add $210 million to development costs (assuming annual costs are constant).[8] Total fuel development costs, as an outer bound to expenditures, could be $900 million. As an inner bound, if the FFTF is available in 1973 and only four years are required to develop from that point to the first carbide fuel rod, total costs can be as little as $476 million.

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[8] If the problems take much more than five years, there is some chance that other solutions would be sought since there is a limit to how much the government would be willing to spend on R&D without some indication that the project can meet target estimates of costs.

Table 3

Costs for Developing Fuel Elements for the Liquid Metal Fast Breeder Reactor

Category of Expenditures		Fiscal Year (millions of dollars of expenditure per annum)								Total
		1966	1967	1968	1969	1970	1971	1972	1973-80	
Fast Flux Test Facility	(Construction)	--	--	--	--	--	--	--	--	87.5
	(Operation)	4.0	7.2	9.0	10.0	10.5	10.5	10.5	15.0	181.7
EBR-II		6.3	7.2	9.0	9.5	9.5	9.5	9.5	7.0	110.2
Materials Development		25.4*		16.4	16.4	16.4	16.4	16.4	20.0	265.0
										$644.4

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[*] These expenditures are cumulative through Fiscal Year 1967.

The Liquid Metal Fast Breeder Reactor: Expenditures on Development of Core Performance

The performance of the fuel core -- the collection of fuel rods making up the critical mass -- is not assessed by summing the measured performances of all of the fuel rods. The core has behavioral patterns of its own, such as temperature and pressure surges that could not occur in a single rod, and those patterns have to be understood and controlled. Theoretical calculations of fission rates and the resulting energy release must be experimentally verified before demonstration cores can be designed and built. With fast reactors, especially, the core design must be evaluated experimentally because fission occurs from atomic collisions with fast neutrons at such a rate that loss of control for seconds, rather than minutes, can lead to melting of the core.

The focus of the light water reactor program on thermal fission left important gaps in knowledge as to the effects of fast fission on the core environment, and as to the rate of fast fission itself. Studies are underway to provide more information in both areas. Capture cross sections -- the frequency distribution of absorbed fission particles -- of structural materials are being documented by observations from experiments in the energy range 1 Kev to 1.5 Mev; and the resonance and scattering of particles in both fertile and fissile materials in the energy range from 1 kev to 3 Mev is being observed in the same experiments. The sources of research and the progress are shown in Table 4 ; given that they continue at the present rate, costs are expected to average about $2 million per year for each year through 1975.

The development of such basic nuclear data is to be supplemented with information obtained from the actual use of fuel cores of various shapes and

Table 4

Studies of Fission Processes

Neutron Energy Range	Major Contributing Facility	Present Rate of Progress
3 to 14 Mev	ORNL - 5 Mev van der Graaff Aldermaston 6 Mev van de Graaff ANL 3-10 Mev neutron generator	Poor
.3 to 3 Mev	ANL, ORNL, Harwell, 3 Mev van de Graaffs ORNL 5 Mev van de Graaff	Very good
1 Kev - 300 Kev	RPI + GA linacs ORNL linac	Very poor
100 ev - 1 Kev	RPI, GA, Harwell, Socley Linacs Columbia cyclotron	Very good

sizes. In the development of light water reactors, over 50 critical experiments were employed to measure reactivity effects on reactor materials, to record temperature and density changes, and to verify safety and control procedures for a particular core configuration. There has been no such experience to date with plutonium-fueled cores. The primary facilities needed to carry out such tests are the Zero Power Test Reactors, ZPR III, VI, IX and the Zero Power Plutonium Reactor (with all except this last reactor being adapted to take large plutonium cores).[9]

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[9] ZPR III is a fast critical facility with a 1500 liter capacity. It is the only facility currently approved to investigate cores with large amounts of plutonium. It has been used to perform critical experiments for EBR-I, EBR-II, Fermi, Rapsodie and Sefor.

ZPR-VI and IX are critical facilities designed to investigate the physics of large (3000 liter) dilute fast reactors. They will be used to verify computational techniques and cross sections for large, fast reactors.

(footnote continued)

The costs for these critical experiments can be divided between "operational" and "experimental" components. The Atomic Energy Commission has estimated general costs for operating laboratories and test installations for these purposes to be approximately $3.5 million annually through 1980; costs for fabrication of the fuel to be used come to between $15 and $20 million in this period. The experiments themselves are expected to cost $2.3 million per year, with an additional $[illegible].1 to 1.3 million per year for each series of full size core critical measurements.

Complementary to these studies in reactor physics and core operation are studies in "safety." Reactor safety analyses are mainly administrative operations which coordinate and evaluate major core experiments for their safety, but they include safety studies of hypothetical fast reactors as well.[10] Most

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ZPPR, now under construction and expected to be in operation by late 1968 or early 1969, is designed to handle plutonium critical assemblies as large as 3000 Kgs; it will be sufficiently large to reproduce to scale the core for a nuclear plant with 1000 MW_e capacity. Hence it will be extremely useful throughout the LMFBR program.

[10] The Atomic Energy Commission Authorization Hearings for Fiscal Year 1968 show the following forecasts for expenditures on reactor safety programs (in millions of dollars).

	FY 68	FY72 - 80
Reactor Safety Analysis	1.5*	1.5
Engineering Safeguards System:		
- safeguard technology	4.0	--
- plant application tests	2.3	--
- standards, codes, specifications	2.3	--
Nuclear Safety:		
- SPERT	2.1	2.0
- power burst facility	2.8	1.2
- chemical reactions	2.5*	2.5
- other reactor kinetics (two phase flow in Na)	4.0*	4.0
Effluent Control R&D	4.7	--
Engineering Field Tests	8.8	--
	35.0	11.2

*Indicates funds related more or less totally to fast breeder programs; the total is $8,000,000.

The statistics for 1972-1980 are estimates consistent with carrying on the 1968 program and with making extensions necessary for the expanded fast breeder program outlined here.

of the safety research related to needs of the LMFBR involves simulation of accidents in the core and observation of reactions to these accidents. Two experimental installations, SPERT and PBF, are currently doing water reactor tests of this type, and can be used for LMFBR experiments as prototypes of this system approach development. The costs of operating them for this purpose will be approximately $8 million per annum for the years 1969 through 1972, and $11.4 million each year thereafter to 1980, for a cumulative expense of $123 million. They should be charged to the LMFBR program (while effluent control is not, since it is a general reactor problem; similarly, engineering field tests for safety in the AEC estimates are omitted since they are primarily devoted to present light water reactors).[11]

Plans are reasonably complete for development of fuel element and core performance to meet design specifications for a 1980 sodium fast reactor. But what if there are significant departures from these plans? Initial research might show that the fuel element can be put together without extensive refinement of existing materials, so that substantial reductions in program size and expenditures are possible. Program plans might also be altered because of poor results at the early stages -- demonstrating the need to fabricate

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[11] The development of an economic fuel cycle does not stop with achievement of a reliable and efficient fuel core. The fuel elements have to be fabricated in volume, and reprocessed after coming out of the core, as well. According to General Electric analysis, five alternative approaches to fabrication and reprocessing have been identified for oxide fuels alone (GEAP-4418, page 4-18). These have to be tried by actually processing fuel and then observing its performance within an experimental or demonstration core. The most promising approaches would be carried through to preparation of working cores. The former preliminary steps would most likely cost $1 million from 1964 to 1980, and the latter would cost $15 million in construction of small scale alternative manufacturing plants.

exotic new cladding materials -- so that costs have to exceed and performance has to lag expectations. The desired development most probably can be achieved at some level of costs even after repeated failure, so that the important implication of changes in plans is the departure from expected research costs.

The engineering analyses and reviews of the fuel part of the intensified fast sodium program seem to conclude that the program either will be followed closely or else be departed from significantly.[12] The first round of observations of prolonged irradiation might show that oxide rods are capable of attaining the desired burnup with sodium outlet temperature at 1200°F, so that there is no necessity for an extensive uranium carbide program. In contrast, five years of irradiation experience could lead to the complete abandonment of uranium oxide fuel, because of exceptionally poor longevity or buckling of cladding materials. Each extreme seems as likely to result as does the design program; but the first would cost $65 million less, and the second -- given no carbide experience, and the necessity for completing the program in uranium carbide development five to ten years earlier -- would cost $90 million more.[13]

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[12] Cf. three analytical reviews of fuel development problems:

1) General Electric, Liquid Metal Fast Breeder Reactor Design Study, GEAP 4418, Part 4 (January 1964).
2) Argonne National Laboratory, LMFBR: Summary of Preliminary Plans, (March 1967).
3) American Nuclear Society, National Topical Meeting on Fast Reactors (April 10-12, 1967).

[13] The estimates are constructed from first deleting carbide development from the program entirely, and second increasing both its rate and scale so as to make uranium carbide rods the only fuel source for fast reactors in 1980. The numbers are subtractions from or additions to the program expenditures in keeping with the planned time schedule, as shown in the text.

The Liquid Metal Fast Breeder Reactor: Components and Systems Development

An exception to the statement that fuel development is the major LMFBR problem could well be raised by those having had experience operating liquid metal cooled reactor systems. Such experience has shown that fuel cores produce energy reliably, but sodium heat-transfer networks are not operable in a large number of cases. There have been problems with materials, with coolant fouling and radiation, and with instrumentation for detecting these problems -- all related specifically to the properties of sodium. The heat transfer system may be more of a challenge for development than oxide and carbide fuels.

Sodium absorbs heat to such an extent that the reactor outlet temperature can easily reach the target of 1200° to 1400°F without pressurization; yet it is solid at normal temperatures, so that reducing temperatures can have the positive effect of interrupting flow where part of the plant is shut down for maintenance. There are disadvantages in using this coolant. At target temperatures, components and piping operate at red heat at all times and undergo thermal expansions and contractions from temperature transients of as much as 800°F. Sodium is explosive in air or water at these high temperatures and absorbs so much radioactivity that piping materials deteriorate. An accident opening the primary heat transfer subsystem would present both safety and health hazards.[14]

The most serious of the materials problems is the development of piping or containment metals that can withstand thermal stress and the sodium-induced corrosion. The extent of the problem is typical, as are the procedures for

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[14] Cf., American Nuclear Society, op.cit., pp. 6-37 to 6-56.

solving it. Stainless steels have been used successfully when temperatures have been limited to 1000°F but not at the design target of 1400°F, so that new alloys may have to be produced to gain the additional 400°. The first experiments in this direction will upgrade the known steels, and the second and following experiments will search for an appropriate material in the more exotic metals if the first round does not succeed.

After some progress has been made on materials, there is the problem of components both for the reactor and control of the system when it is in operation. Pumps, valves, seals, bearings, piping, heat exchanger piping, and purification equipment all are subject to high temperature sodium. Most have been constructed and operated at 90 per cent of design temperature for 50 per cent of design size; new larger units have to be produced to test for reliability in operating behavior at the required higher performance standards. The problem again has an either-or solution: build units that have the same design as those now used, but that are larger and of higher quality materials, or start over with an uncertain design that has a higher performance target at the desired level of operation. For example, the mechanical seals used in presently-operating sodium reactors have a lubricated face, and can serve as the base for a development program to 1980 design levels; but "the use of a lubricant requires an elaborate design to prevent the lubricant from contaminating the sodium ..."[15] not found in a dry-face seal so that the latter type is likely to have more reliable long-term performance. The first is well developed, requiring some enlargement and improvement in operating life, but

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[15] American Nuclear Society, op.cit., page 8-26.

the second has never been adapted to a sodium environment, so that it has a lesser chance of achieving better design results.

Decisions on routings will be made during experiments at a complex of component test facilities operated by the Atomics International Company in California. First-of-a-kind pumps, valves, piping arrangements, et.al. will be installed in a sodium loop fired by a gas burner;[16] as the ability of new pumps to discharge at 120,000 gallons per minute is demonstrated, for example, the evidence will mount up on the need for more extended research into entirely new types of drive mechanisms. The approach on all components is to build one and test it under conditions as similar as possible to those in the 1980 reactor -- and then discard it for another model when it fails to work under those conditions. The costs of the Atomics International facilities, and of other component development projects that are part of the experimental approach, can be expected to be extensive. They are shown for the fiscal year 1968, as estimated by the Atomic Energy Commission, in Table 5.

The first category of expenditures covers the operation of the sodium components test installation (SCTI) at the Atomics International California center. This operation should complete testing of new models of particular pieces of equipment by the time that 300 MW_e prototypes are operational, since the 300 MW_e plants have to use components appropriate for the 1000 MW_e plant in order to be in line with the large 1980 demonstration. Since there are numerous contending designs for the sodium subsystem, each of the first few prototypes should have different heat exchangers, pumps, etc., which have

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[16] Cf. American Nuclear Society, op.cit., page 8-35, et.seq.

Table 5

Components Development Costs in 1968

Category of Development	Millions of Dollars
1. Sodium component testing	3.26
2. Sodium technology development	2.70
3. Heat exchanger development	2.79
4. Pumps, valves and piping development	1.45
5. Reactor mechanisms and sodium instrument development	1.30
6. Fuel handling mechanisms development	.60
7. Reactor vessel and internals development	.40
	12.50

Source: Atomic Energy Commission Authorization Hearings, Fiscal Year 1968.

previously been tested so that the sodium installation has to be large enough to carry out tests on a number of loops at the same time. The costs shown for this first fiscal year will have to be greatly expanded.

There are a number of research projects preliminary to finding all of the best components. Expenditures on these are shown in Categories 2 through 7 of the Table. The technology of sodium has to be investigated in more detail throughout this period (shown as Category 2). The explosive sodium-water reaction has never been thoroughly explained or understood; in addition, problems of mass transfer and other impurities in sodium remain to be solved before commitments are made to metals to be used throughout the 1000 MW_e demonstration plant. A heat exchanger must be made that will work without experiencing extensive materials deterioration during the later years in the plant lifetime; this is a project with priority over the testing of other components because

the test loops themselves have to have such exchangers (Category 3). These two types of projects are related: problems in the exchanger such as extinguishing sodium fires, containing sodium-water reactions, and containing rapid thermal transients, can be dealt with when more is known about the behavior of sodium. The components themselves must be beyond the design stage and standards for performance must be established before testing can begin (Categories 4, 5, and 6). This phase of the program should be completed in five to seven years, with the tests verifying the usefulness of the available hardware towards the end of that period.

The critical component development problems are in providing safe and low cost containment for the reactor. The vessel and shielding of fast reactors built to date have made up a "complex structure which generally has been more difficult to analyze than the containment structures used for the water reactors"[17] because of special requirements for preventing the escape of sodium. The apparent means for achieving a low-cost and safe structure seems to be to divide it into two parts, one enclosing the core and primary heat exchanger, the second enclosing the entire plant, with both of reinforced concrete. This would seem to work, but there is still the need to control cracks and leaks with surface coatings yet to be developed and the vessel's behavior when internal heat exceeds design conditions is undocumented. This behavior affects the economic advantage of large fast reactors: "The significance of such cannot be over-emphasized ... this can mean the difference between a row of

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[17] American Nuclear Society, Ibid., page 6-46.

7/8" diameter bars spaced at 10" to 12" and a double row of 1-1/2" bars spaced at 4" to 5" on center."[18] The research program to provide answers here begins modestly, as shown in Table 5, but will have to expand to more than $1 million per annum in the period stretching to 1980.

The total costs of components development are estimated from these 1968 costs, and from the requirements for expanding this first year's outlay to meet the design goals with moderate success. The experiments and tests are not expected to fail, nor are they expected to show target performance from moderate upgrading of working components in the present EBR or Fermi reactors. Steady but only moderate success would consist of achieving target performance with the third or fourth test components, and with new metals or radical designs for this last component in a quarter of the cases. The costs of this level of achievement generally are expected to add up to the annual expenditures shown in Table 6. .

After the many components have been developed to achieve approximations to reasonable technical performance in a test facility, they must be integrated to make up systems of their own, if they are to demonstrate economic operation in a convincing way. There are currently four core configurations in the design studies, two sodium circulation designs (one of which sinks the entire core in a pool of sodium) and at least three different heat exchanger designs so that there are possibilities of at least twenty-four prototype plants. All of them of course will not be constructed: the test facilities should enable theoretical evaluations of heat exchangers to lead to an optimum design prior to any construction. But this is not the case for

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[18] Ibid., page 6-51.

Table 6

Components Development Expenditures

Fiscal Year	Millions of Dollars
1968	12.5
1969	16.0
1970	16.0
1971	14.5
1972	14.0
1973	12.0
1974	12.0
1975	12.0
1976	12.0
1977	12.0
1978	12.0
	145.0

Source: Extrapolation of the program shown in Table 5 on the basis of needs and success rates in meeting these needs as described in the text above.

the four core types and two sodium circulation designs -- some of the theoretical research should allow selection of a few from the several promising concepts, but a number of alternative prototypes will be needed to choose the final LMFBR design. There is no way to evaluate a core's behavior short of building a model of that shape and size.

One of the major problems with liquid metal designs is that there are significant differences in the fission behavior of small fuel cores and the larger cores for the 1980 full scale reactor. Therefore, small experimental cores are not adequate for proving the worth of concepts, and there is a premium on prior analysis before construction of large prototypes. The costs of the prototypes themselves will exceed conventional nuclear costs

for comparable sized plants by no more than 50 per cent, at least at the forecast stage, because otherwise there will be no cooperative development programs. For a 300 MW_e prototype, the present generation of plants sets a limit on capital costs of about $300/KW; for a 600 MW_e plant, the upper limit on cost would be $225/KW. These research costs are expenditures beyond the costs of present water reactors, and come to approximately $30 million for a 300 MW_e plant and $45 million for a 600 MW_e plant.

Total requirements for "proving out" fast reactors could come to as many as four to five large-sized prototypes, given the number of possible and equally likely core configurations and of methods of circulating sodium. Out of these four at least the most promising might be built at the 600 MW_e prototype size so as to gain important working experience with the full-sized core. The schedule for the 1980 plant shows that it is necessary to construct one prototype per year from 1969 to 1972 at the 300 MW_e level, one 600 MW_e plant in 1976, and the demonstration plant itself to choose the best design in the late 1970's.[19] Each prototype should take six years from contract commitment to operation, given that LMFBR components have higher priority with reactor manufacturers than do present conventional reactor components. This is a tight schedule. Commitments for the 600 MW_e plant have to be made before there will be much operating experience with the first prototype. But this

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[19] The AEC has tentatively indicated interest in three prototypes spaced several years apart. It is conceivable, however, that four might result as a result of utility pressure to support a more competitive environment. If GE were to obtain support for the first prototype, for instance, it is likely that Westinghouse would press a second group of utilities to obtain AEC funds for a second prototype very shortly thereafter. Combustion Engineering, Atomics International, and Babcock and Wilcox would press for other prototypes on the quite valid grounds that to be left out at this stage is to be left out entirely.

is the quickest and cheapest path to moderate success with systems. Total costs would be $230 million, as a result of expenditures of $30 million each year from 1969 to 1972, $45 million in the combined years of 1973-1976, and $65 million for the research on the demonstration plant in 1980.

The whole of the program to produce a sodium fast reactor poses more alternatives than can be known and counted at the present time. Knowledge will be accumulated from other research projects in the non-energy industries, and some of it may well offer cheap new means of cladding fuel that can allow termination of some of the present research on stainless steels. But other new findings elsewhere may make it clear that present promising research approaches will ultimately not work -- so that some of the present alternatives will be foreclosed, as well as rendered obsolete. What is known at the present time is shown in Table 7: there are more than two designs for each component and subsystem, and there has been some progress achieved towards 1980 standards of performance. With middling success -- an inability to reach required levels of performance after testing the first model of the first design, but an ability to avoid having to start anew with an unknown material -- then costs of research are expected to reach $1.6 billion before the 1000 MW_e demonstration reactor is in operation in the early 1980's. But with equally probable (a) high levels of success, or (b) poor results at each step, costs can vary from $1.2 billion to $2.2 billion.

Table 7

DEVELOPMENT GOALS AND COSTS FOR THE LIQUID METAL FAST BREEDER REACTOR

OBJECTIVES	PRESENT POSITION	PROGRAM TO TERMINATION OF PROJECT	EQUIPMENT REQUIRED BUT NOT YET IN PLACE	COSTS (millions of dollars) LOW COST	DESIGN COST	HIGH COST	TIME
(1) a) Fuel Rod Performance. 100,000 MWD/tonne at 800-1200°F for coolant, and producing 5 to 25 KW/foot.	a) Irradiation of fuel elements in EBR-II, SEFOR, to 100,000 MWD/tonne is underway. Initial results show "promise" beyond 50,000 MWD, but experienced "high" failure rate. Present facilities extremely limited.	a) Continuation of irradiation in existing fast facilities and FFTF, three-year exposure of some fuel elements required.	Fast Flux Test Facility (FFTF)	476	645	900	1980
b) Fuel Clad Performance: Develop [illegible] to take radiation damage and transient over-power to level required for (a).	b) Initial irradiation of clad in fast flux to 50,000 MWD/T concluded. Treat tests of power transient effects underway, with initial tests concluded successfully.	b) Documentation of resistance of 316SS to mass transfer in dynamic sodium. This is a six-year project (two for construction, four or more for testing).	Safety Test Facility (STF)				1975
Other materials to show same level of promise as stainless steel now shows.	Irradiation in thermal flux beyond 125,000 MWD/T achieved successfully.						
(2) Fuel Processing: Development of a single process, of the five proposed, that produces mixed-oxide fuel to performance. Experimental research to establish cheapest recovery methods.	The Purex process on a single cycle works for low radiation.	Small scale to pilot scale experimentation with all processes.		30	30	30	
(3) Subsystem: Core Performance a) Determination of critical mass. b) Reactor geometry for maximum neutron economy and stable operation. c) Energy spectrum under wide variety of conditions. d) Impact of bubbling and fouling in the core. e) Meltdown characteristics.	a) No Pu-fueled fast assemblies in operation; no material holding a fast assembly together has been developed. c) Reactivity coefficients differ by factor of two or three. c) Irradiation program underway in 1963 adequate, but time is needed. e) Mark II core of EBR-I yielded meltdown information.	a) Testing of assemblies for criticality; mockup of 1,000 MW_t core with control configurations. c) Measurement of reactivity and Doppler in one isotope at a time in various neutron spectra, all in large critical facility with heated core. Doppler measurements in SEFOR and FERMI. d) Data from TREAT tests and FFTF on sodium voiding.	ZPPR - 6/28 - Mockups and clean critical expts for a), b), c) ZPR-6, ZPR-9, Reactivity Coefficients for c). Fast Spectrum measurements for c). ORELA - 1/69 Fission and Cross Section measurements for a),b),c),d) ANL NEUTRON GENERATOR - 1/69 for cross section measurements, a) - d).	176	241	331	1978
(4) Subsystem: Components and the Behavior of Sodium Coolant a) Components such as pumps, heat exchangers, valves, seals and bearings, purification equipment, liquid metal instrumentation, contaminant vessels. b) Data on sodium-to-steam heat transfer characteristics of the system. c) Data on Basic Sodium Properties, data on basic sodium-water-air interaction.	a) Materials are adequate for near term and most long term objectives. b) Operating experience with sodium up to 1,000°F. Uncertain as to operating characteristics of primary loop between 1,000°F and 1,250°F. c) Experiments with boiling at AI and APDA.	a) Sodium components development program carries these to the prototype. Test loops to evaluate corrosion and heat transfer (SCTI). b) Measurements of creep rates, fatigue stength at 1,400°F. b) Experiments to test proclivity of system to self-weld above 1,000°F. c) Develop sodium purification methods and knowledge of material compatibility with total system.	STF (6/73)	115	145	265	1975
(5) System Development Instrumentation and control of the entire system.	FERMI shut down. EBR-II in operation. SEFOR in test and initial operation.	Thermal cycle analysis to establish temperature rises throughout the system, and to set optimal core thermal power. Optimization of Fuel Cycle.	FFTF STF	250	300	375	1983
Operation of prototypes in accordance with design specifications.		Computer simulation of system, to include reactivity coefficients; simulation of accidents. Integration of components to achieve system capability. Construction of prototypes to test subsystem integration, and to estimate costs of operation.	4 Prototypes 1 Demonstration Plant	180	230	305	
				$1,227	$1,591	$2,205	

The Steam Cooled Breeder Reactor: Core Performance

The steam cooled breeder reactor may provide a near term opportunity to utilize the plutonium produced by the reactors installed in the next five to ten years. This type of reactor may be viewed as an extension of present technology, or as the subject of an independent program to be developed on its own; either way, the forecast development period is shorter and the expenses less than those for the liquid metal reactors. The first view emphasizes the similarity of technique in cooling, given that steam is circulated through the core in boiling water reactors and in the steam-cooled fast breeder reactor. The second emphasizes that new problems arise with coolant circulation, and with core performance, when temperatures are increased beyond those best for thermal fission to those for fast breeding.

As with all major reactor programs, fuel element research is the largest single component of the program. There is, at this moment, little relevant experience with a fuel element in a fast flux environment with steam as the coolant. What has been done has been promising. The most helpful findings have been those eliminating some candidate cladding materials. Experience with EVESR, a thermal reactor experiment, showed that stainless steel cladding materials were very susceptible to corrosion in a steam environment, but that nickel clads, particularly inconel-600 and incoloy-800, were not as prone to corrosion. Thus the candidates for fuel element cladding material have been narrowed down before large scale irradiation experiments begin.

The properties required for fuel elements in a steam environment can be compared with those for a liquid metal fast breeder. The fuel should be stable over burnups of 100,000 MWD/T with operating temperatures at approximately 1400°F, both of which are close to sodium conditions. However, there

is a great difference in operating pressure in the two reactors: in the pool-type sodium circulatory system, the pressure is essentially that of the sodium, whereas for the steam reactor, pressures of 1500 p.s.i.a. and above are contemplated. As a result, either there has to be a pressure-equilizing gas filler in the fuel rod, or the cladding for the fuel rod in a steam reactor must have much greater physical strength. The stainless steel cladding designed for the liquid metal reactor would probably collapse in the steam pressure conditions if a hot spot developed due to non-uniform fuel expansion; but the nickel alloys have the strength to withstand these particular stresses. Consequently, interest is centered on inconel and incoloy for steam fuel cladding, and the development program is not the same as that for the liquid metal reactor's fuel element.

The programs do overlap to a great extent, however. The oxide, carbide and alloy fuels are as promising for the steam as for the sodium reactor, and the burnup and transient studies of each of the possibilities are the same for either program (so that Experimental Breeder Reactor II, the Fast Flux Test Facility, and the Safety Test Facility are useful for both development programs). The parts of development programs which do not overlap are concerned with exposing the fuel elements to the relevant coolants. An experimental steam-cooled reactor would be needed for such irradiation studies. Two major facilities -- an FFTF and an experimental steam reactor -- comprise the basic construction for a separate steam program -- if steam is the only such program. A plan to produce a fuel element for this reactor alone might take the following steps: test fuel rods in EBR-II, until the FFTF is ready; continue testing in the latter facility for roughly three to four generations until some were ready for burnup evaluation in the steam-cooled environment. For these

last experiments, the rods would be installed in the Experimental Steam Cooled Reactor (ESCR) since this first steam fast reactor would be ready for operation in 1972. Another three to four generations would produce both fuel alloy and cladding material which would lead to burnup at the hoped-for level of 100,000 MWD/T. Assuming that the program operates in this fashion, the costs for fuel element development are those shown in Table 8.

Table 8.

Fuel Development Costs for the Steam Cooled Reactor

A. The Fast Flux Test Facility

Construction -- $87.5 million

Design and operation ($ million)

1968	1969	1970	1971	1972	1973-1980	Cumulative
9.0	10.0	10.5	10.5	10.5	15.0	170.5

Operation of EBR-II

1968	1969	1970	1971	1972	1973-1980	Cumulative
9.0	9.5	9.5	9.5	9.5	12.0	59.0

B. Fuel Development

1968	1969	1970	1971	1972	1973-1980	Cumulative
14.2	16.4	16.4	16.4	16.4	20.0	240.0

C. Experimental Steam Cooled Reactor (ESCR)

Construction -- $30 million

Operation

1968	1969	1970	1971	1972	1973-1980	Cumulative
1.0	2.0	4.0	4.0	7.0	7.0	74.0

These are the costs for a fuel development program that stays fairly close to expected performance levels at each stage; they come to a total of more than $661 million for this part of the project alone. They will be much less if the Fast Flux Test Facility turns out to be available earlier than anticipated and if it takes a shorter time than expected to develop a fuel element once the FFTF is operational. The FFTF might be available in 1973 rather than 1974, so that the development period is 5 years rather than the presently estimated 6 years; then the whole fuel development program could be finished after only 9 years of operation (including 4 years of testing to show that the elements are capable of burnups of 100,000 MWD/T). The costs of construction could be as low as $67.2 million for the FFTF (the low estimate of the Atomic Energy Commission) and $500/KW, or $25 million for the experimental steam cooled reactor; then the low estimate for fuel cost development would be $556 million with construction costs $25 million less than expected and operating costs $80 million less during the shorter running period of this part of the project.

Rather than achieving fuel burnup standards at an early date, the program could as well run into difficulty. The Fast Flux Test Facility itself could be subject to long delays from the installation of faulty equipment and might take several more years than estimated to produce the required levels of irradiation. If the FFTF's costs are 50 per cent more than expected, both to correct the causes of delay and as a result of having to postpone other parts of the program, then the added total costs could be as much as $218 million. This would accrue from construction expenses $18 million more than expected, operations expenses for the FFTF and EBR-II $100 million more than expected, and fuel development costs another $100 million more. Such results would bring total fuel development costs to approximately $879 million.

But what if there is an ongoing liquid metal research program at the same time as this steam cooled program? In that case, the cost of fuel research for a steam cooled breeder would not be as great as total expenditures for operating EBR-II and building-operating the Fast Flux Test Facility, but rather only those expenditures for the particular coolant loops in these facilities dedicated to development of the fuel cores for the steam environment.[20] The minimum number of loops would be two, and each would cost $10 million along with $1 million per annum to operate. Special work would have to be done on the cladding materials needed for the steam environment, and tests in an experimental steam cooled reactor would have to be undertaken in the same manner as in the absence of a liquid metal program; the costs of the first might be as much as $3 million a year, while the costs of the second are as shown in Table 8. The total costs of the incremental program in steam breeder reactors are then equal to $189 million. They may be as low as $171 million, if the time span of research is shortened, and as high as $239 million if research delays and disruptions add $5 million to the costs of each FFTF loop, $15 million to the cost of fuel development, and $25 million to the cost of operation of the experimental steam cooled reactor.

The Steam Cooled Breeder Reactor: Development of Core Performance

The make-up of the critical mass to form a fuel core has always been subject to *a priori* analysis; predictions have been made of performance

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[20] This allocation of costs is a particular response to a precise question; if the liquid metal program is not precedent, then the costs of additional loops are not all of the expenses allotted to the steam project.

since the first core was constructed in the squash courts at the University of Chicago midway through the Second World War -- which always have been accompanied by error either large or small. Performance tests have to be made by constructing a full scale model of the fuel and then observing the operating behavior of fission under various temperature and pressure conditions. These tests have not been made in the steam cooled breeder program, although they have been approached in experiments with a number of thermal fission cores when steam has been the coolant. The operation of fuel cores under super-heat conditions (as at EVESR, the first privately owned and licensed steam-cooled reactor in the United States, which has been operated as a superheat experimental reactor) has allowed analysis of full-sized cores at temperatures only 200°F less than those sought for the breeder. But there are still differences: "The steam cooled fast reactor has a more compact lattice, higher heat flux, and lower heat transfer coefficient."[21] There is not enough information from thermal reactor experience to go ahead with the construction of prototypes.

The information needed is vital for deciding whether fission is a stable process. Changes of reactivity with respect to departures from design temperature, coolant flow rate, or pressure have to be negative to maintain control of the reactor; that is, if reactivity decreases in the presence of a large temperature surge, then there is no possibility of a reactivity-induced, second-round temperature surge. Core analyses for predicting such coefficients are needed for fast neutron conditions regardless of the coolant, and special core analyses are required for this particular coolant as well. The facilities required are steam versions of the Zero-Power Plutonium Reactor series.

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[21] American Nuclear Society, op.cit., page 7-35.

The sequence that leads to successful operation of a core in the steam version of the ZPPR is as follows: (1) collection of data on the frequency distribution of neutron energy under steam conditions; (2) construction of a small scale core for experiments with size, shape and temperature; (3) preparation of predictions on the behavior of the full-sized core under normal and simulated disaster conditions. The first step has yet to be taken: most of the information now available on neutron flux is either for water or steam cooling when there is a moderator present. It can be accomplished with the devices now used to observe the frequency distribution of neutron energy (the neutron "cross section") in light water reactors, but, after such devices have been transferred to fast reactor research, it takes considerable time to complete this work. The second step follows the first because the fission energy of the neutrons determines the size and shape of the critical mass; but even after substantial neutron energy information is available a number of core conformations will have to be considered and models of some of them constructed. An external steam generator must be built, as well, to provide the steam environment in which to test these models. The last step consists of preparation of the zero power reactor itself, and experiments with the reactor, for the purpose of reproducing a range of conceivable operating conditions on a small scale and then observing the resulting fission behavior. Much of this research has to be centered on core temperature and radiation rates when there is channel blockage in the steam system so that there is "loss of coolant" in the core. There are obviously large temperature surges in such an event, and research is required to assess the tolerance of the fuel elements to such surges without melting the cladding material.

This stage of core development is concerned mostly with safety devices and other techniques to maintain control of nuclear fission. The effects of "loss of coolant" can be minimized by incorporated an auxiliary steam circuit capable of instant startup and emergency flooding of the core; the question is when during a temperature surge such a circuit should be used. The answer is "when the core is about to melt" so that extensive data must be collected on uranium reactions to extreme temperatures to find that melting point. A Safety Test Facility is the locus of such research in the Liquid Metal Fast Breeder program, and should also be the source for the same work on the steam-cooled fast reactor.

The core performance research is, in most respects, the same for the steam and sodium programs. Experiments with fuel behavior when there are sodium voids does not differ in kind from those under steam blockage and they both take place in the same zero-power and safety-test facilities. The costs should be the same for the two programs if they are this similar; if they are equal to $241 million for sodium fuel development that progresses at a middling success rate, then the costs with the same equipment in the steam fuel development program should be at this level. They could be as low as $176 million with early and distinct success in finding the design for the high-performance core, and as high as $331 million if the worst conceivable results are experienced.

If the steam program is an addition to that for the sodium fast reactor, then costs could be much less than shown here. Steam research could be somewhat delayed and put behind the work to be done on sodium reactor fuel; then steam expenditures would be limited to the added costs of operating the sodium

zero power and safety test equipment for the extra steam experiments. Critical experiment analyses for the sodium program, which here include "cross section" measurements, have cost between $1.6 and $3.2 million in recent years and are expected to continue at this level of expenditure. The incremental costs for core spectra measurements under steam conditions would be about $500,000 per year, and experiments with a mockup core would cost $600,000 to $800,000. It is reasonable to assume several core spectra measurements per year, and at least one mockup, so that the costs of the first and second steps of an incremental steam program are about $1.8 million per year; this level of support would be needed through 1975 (which is somewhat lower than that for the comparable period for light water reactors, reflecting the fact that more is known today). Comparable reduced expenditures could be expected for the third step -- that part of the program devoted to theoretical analysis -- as well. Even though a part of theoretical research should be of general use to either coolant systems, there is no substitute for a zero-power reactor devoted exclusively to the steam environment; a total of $1 million per year seems reasonable for work in such a steam ZPPR facility. Finally, costs for safety analysis in an incremental operation are closer to those for the single steam project, because here the programs diverge; except for the development of fast control mechanisms, projects to observe fission behavior at limit temperatures are different because the coolant significantly affects such behavior. The costs are assumed to be twice as great for two projects as for one. Then total costs for core performance development as an incremental project are as shown in Table 9.

Table 9

Core Performance Research for the Steam Cooled Fast Breeder Reactor as an Addendum to Sodium Research (millions of dollars)

Research Category	1968	1969	1970-1972	1973	1974	1975-1980	Total
Basic Nuclear Data	1.0	1.0	1.0	1.0	1.0	0.0	7.0
Integral and critical experiments	1.8	1.8	1.8	1.8	1.8	1.8	23.4
Theoretical studies	1.0	1.0	1.0	1.0	1.0	1.0	13.0
Safety	8.0	9.0	10.0	11.0	11.0	11.0	135.0
							178.4

This, again, is an estimate of expenditures to achieve target performance if all steps are taken after "normal" or "middling" experience with the specific required projects. Low costs, assuming that the needed physics information is developed in two years less time than originally expected, would be $31.6 million less. High costs would follow from having to take five additional years to collect this information and carry out the ZPPR experiments; they come to $74 million more than the estimate shown in Table 9. All of these results are conceivable -- perhaps equally likely -- so that the range of results should be from $147 million to $252 million for an incremental steam core performance program.

The Steam Cooled Breeder Reactor: Components and System Development

The design for a fast reactor with steam as the coolant is both simple and familiar. Since this gas is routed from the core directly to the turbine, and then back from turbine to core through a condensor, there is no complicated

or novel heat exchange system. The unfamiliar component in the design, the Loeffler boiler to pre-heat the steam, has been used in Europe for many years. New steam circulators are also required, since steam is transmitted from the Loeffler boilers to the reactor, but this is a matter of upgrading existing pumps and valves. Several small items such as quick-acting valves and control rod mechanisms have to be upgraded to meet the more exacting standards of fast reactors. But all of these involve straightforward development and design improvement rather than basic research with uncertain results.

Loeffler boilers are direct extensions of the steam systems used elsewhere at the present time. Similarly, steam circulators that can move up to 1.4×10^6 lbs./hr. have been in use for some time; their size is twice that required for the experimental steam reactor but only 40 per cent of that needed for the steam demonstration reactor. Altogether approximately $6 million would seem to be a maximum expenditure for development of the Loeffler boiler and the steam circulator; and the remaining smaller components should cost no more than $4 million to build and operate successfully. Therefore total component development costs are about $10 million if this part of the program is neither surprisingly simple nor difficult. In the best of circumstances, costs could be half this amount given that there are no failures of first models at all (three model failures would seem to be the number that normally can be counted on to occur). If excessive difficulties are encountered, and six models of the two unfamiliar components are needed before final success, then $30 million would be required.

The first system from these components, of course, is the experimental reactor at zero power for testing core performance. Once this is built and operated, the technology of a complete plant has been demonstrated; costs

must be lowered from those on the first round by building 300 MW_e prototypes whose components are appropriate for the 1000 MW_e commercial plants of the future. Such prototypes establish both the reliability of individual components, and of the systems composed of these components -- including alternative designs, such as pancake and modular core geometries. Data collected on operating costs and production are important for deciding which is the best of the designs for use in the full-scale demonstration plant. Given present, fairly extensive knowledge of steam subsystems, it seems likely that only one experimental reactor but several prototypes would be needed. (Also, few companies have come forward with radical designs for steam breeders, so that it is unlikely that several experiments would be actively pursued). The most extensive program within reason would rely on one experimental reactor and three prototypes (300 MW_e) to reach the 1000 MW_e demonstration plant size.

Expenditures on the Experimental Reactor have already been included under fuel development costs (since components testing is less important than fuel development at that stage); the expenditures on three prototypes should be in the \$250 to \$300/KW range, approximately of the magnitude of those on the Yankee or Dresden reactors at the same point in the light water program. If actual construction costs turn out to be \$300/KW, with development outlay making up the difference of \$100/KW between these costs and those of a conventional nuclear plant of that size, then prototype development costs are \$30 million per plant, and \$90 million for the three. In addition, the project can be expected to be responsible for a part of the costs of the first demonstration plant: total costs will likely be about \$150/KW or \$150 million,

of which 1/5 or $30 million will be expenses not related to this plant alone but to advanced development of all plants of this design. Finally, another $30 million should be spent on improving on designs for another round of components for the demonstration plant. Thus total expenses for system development should add up to $150 million, if there are only fair-to-good results from operations with this limited number of reactors.

It is possible to estimate high and low costs for this part of the project. The low costs could follow from doing as well in this case as earlier: component research came to only 14 per cent of total project costs in the light reactor program which if achieved once again would reduce construction costs to $88 million. If other costs are also reduced by $10 million, as a result of fewer design studies and fewer experiments, then systems costs would be only $78 million. On the other hand, the highest reasonable costs would be incurred if support had to increase to 50 per cent of the costs of cooperative projects, or an additional $90 million, and had to include additional expenses of $30 million for design changes; in that case, research costs could exceed $270 million for reactor plants alone.

The spectrum of research goals and possible costs for the entire steam cooled program is shown in Table 10. A first review shows remarkable differences: it seems more narrow than for the liquid metal program, the number of new components and techniques is limited, and so are the costs. If a single reactor project is to be undertaken, then the steam cooled fast breeder can most likely be made successful for between $800 million and $1,500 million dollars -- costs that are $500 million less than for the sodium breeder. If the sodium project were also going ahead as planned, the steam reactor concept

Table 10.

THE STEAM-COOLED BREEDER REACTOR DEVELOPMENT PROGRAM

OBJECTIVES	PRESENT POSITION	PROGRAM TO TERMINATION OF PROGRAM	EQUIPMENT REQUIRED BUT NOT YET IN PLACE	RESEARCH COSTS (millions of dollars)			TIME
				LOW COSTS	DESIGN COSTS	HIGH COSTS	
(1) FUEL ROD PERFORMANCE: a) 100,000 MWD/T at 800-1200°F for coolant, producing 5-25 KW/FT.	a) Irradiation of fuel elements in EBR-II, SEFOR, to 100,000 MWD/tonne is underway. Initial results show "promise" beyond 50,000 MWD, but experienced "high" failure rate. Present facilities extremely limited. No experience in a fast steam environment: all tests so far in a sodium environment.	a) Continuation of irradiation in existing fast facilities and FFTF, three-year exposure of fuel elements required, a part of which have to be under high pressure steam conditions.	FFTF STF ESCR (Experimental Steam Cooled Reactor)	556 171*	661 189*	879 237*	1978
FUEL CLAD PERFORMANCE: b) Develop SS or other materials to take radiation damage and transient over-power to level required above.	b) Fuel tested for thermal fission behavior in EVESR steam environment. Inconel-600, Incoloy-800 demonstrated corrosion resistance. Significant failure above 1400°F.	b) Irradiation testing of fuel rods with different cladding materials in steam cooled environment in FFTF, or ESCR.					
(2) FUEL PROCESSING: Develop an economically attractive recovery method.	Pyroprocessing with EBR-II.	Small scale to pilot scale experimentation with all processes.		30*	30*	30*	1975
(3) SUBSYSTEM: CORE PERFORMANCE: a) Critical mass b) Reactor geometry for maximum neutron economy and stable operation. c) Energy spectrum under wide variety of conditions. d) Impact of loss of coolant on safety. e) Meltdown characteristics.	a) No Pu-fueled fast assemblies currently in operation. b) Uncertainty on magnitude of coolant coefficient in large size cores. c) Reactivity coefficients poorly known in fast flux systems. d) No information at present. e) Mark II core of EBR-I yielded meltdown information.	a) Testing of assemblies for criticality, mockup of 1000 MW_e core with control configurations. b) Critical experiments to determine coolant coefficients. c) Measure reactivity coefficients in critical experiments, determine cross sections, etc. d) Tests in STF or comparable experiment facility. e) Theoretical investigation and FARET type experiment.	ZPPR - 6/28 - Mockups and clean critical expts. for a),b),c) ZPR-6, ZPR-9, Reactivity coefficients for c). Fast spectrum measurements for c). ORELA - 1/69, Fission and cross section measurements for a),b),c),d) ANL NEUTRON GENERATOR - 1/69, for cross section measurements, a)-d). STF	176 147*	241 178*	331 252*	1978
(4) SUBSYSTEM: COOLANT SYSTEM: a) Components such as Loeffler boilers, purification equipment, instrumentation.	a) light water reactors utilize most of the equipment required. Materials effects well known.	Components development program.		5 5*	10 10*	30 30*	1973
(5) SYSTEM DEVELOPMENT: Instrumentation and control of entire system. Operation of a prototype in accordance with design specifications.	FERMI shutdown. EBR-II in operation. SEFOR in test and initial operation. EVESR tests computed.	Build 3 prototypes of 300-600 MW_e range to prove component design, safety, economic potential and then a 1,000 MW_e plant.	3 - Prototype Plants 1 - 1000 MW_e Demonstration Reactor	88 88*	150 150*	270 270*	1980
				835	1,092	1,541	
				441*	557*	821*	

*Indicates incremental costs to AEC's LMFBR program.

might well prove out for \$400 million less than these amounts in the \$700 to \$1,500 million range; that is, a second fast breeder in a world of remarkable research success might "prove out" for only \$400 to \$500 million.

The Gas Cooled Fast Reactor: Fuel Element Development

A fast breeder using another gas besides steam has claim, on the drawing boards at least, to consideration as the subject for a large scale development project. Helium has been used as the coolant in thermal reactor experiments and in the full scale demonstration reactor at Peach Bottom, Pennsylvania, so that a fast reactor could be built on the basis of experience with components in a gas environment. The heat exchangers, circulators, valves, etc. needed in a gas fast reactor are identical to those required for an advanced thermal reactor now on order; components developed for this Fort St. Vrain reactor should be ordered for the first experimental helium reactor. However, the gas breeder is not now developed; the same problems are present of fuel element development, core performance analysis, and system development, which characterize the steam breeder. There are additional disadvantages in a lack of operating experience in the United States, so marked that the gas fast concept is behind others at present and would probably require the longest time period from the beginning of fuel rod research to the completion of a 1000 MW_e demonstration reactor.

Work on the fuel element begins with a design for the fuel pin and makes important progress when a clad material compatible with both fuel and coolant has been chosen. It seems conceivable that knowledge of fuel pin behavior obtained in the liquid metal fast reactor program is useful for pin

design for a gas fast reactor. If this is true, then development of fuel for the latter system will concentrate on testing of clad in the helium environment, by installing gas loops in the Fast Flux Test Facility and by constructing a gas cooled fast reactor experiment (GCFRE) for a full sized core in a helium circulation system.[22]

Consider a program in which the gas fast breeder is the only concept on which research takes place. The fuel pin research would proceed as in the sodium fast reactor project since this part of the project should produce results applicable to any fast reactor. Corresponding work on cladding materials would be held back at first by the lack of a fast flux helium environment in which to test a complete fuel element; rapid construction of FFTF (for closed loop testing of elements) and GCFRE (for both fuel element and subassembly testing in the appropriate gas environment) would cost $87.5 million and $30 million respectively. The FFTF would become operational in 1974, as in the liquid metal program, and the GCFRE with about half the proposed capacity of the FFTF (300-500 liters vs. 880) should be available four years after submittal of the preliminary design. Operating costs alone could be $3 million and it is expected that costs related to preparation and analysis of tests would be $3 million more.[23] The last of the three major facilities

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[22] The GCFRE would take three years to produce burnups of 100,000 MWD/T, compared to four years in EBR-II but one year in FFTF. It could very well be used to develop a reliable fuel, but not to explore a large number of alternative fuels in a short time interval. The limited search seems reasonable given the present state of gas technology, but could result in a long delay in the development of the lowest cost fuel element (except by a chance choice of the best material first).

[23] This is comparable to Atomic Energy Commission spending on one major fuel program, such as uranium carbide, at the present time.

that would be used is Experimental Breeder Reactor-II. When the other two are available, there is little need for EBR-II, although its test positions could be helpful for pre-testing the proposed fuel assemblies; operating expenses for this reactor could be as low as $5 million annually after 1974, as a result.

With all efforts focused on problem solving in the helium gas environment, the period for fuel development should be at least four years longer than for sodium breeder fuel. The gas project, at best, has a smaller number of test positions, which implies slower accumulation of test results. Consequent from this longer research period, and from the need for the highly specific GCFRE, expenses for fuel development, shown in Table 11, are $150 million greater than for a similar sodium program.

An optimistic, low estimate of fuel costs follows from assuming that the element is developed on a sodium-type time schedule -- or four years earlier than most likely -- and that the FFTF costs are in keeping with the low Atomic Energy Commission estimate of $67.5 million. These assumptions are plausible -- at least they are held by those that would be on AEC contract for a gas fuel program. The results would be costs $68 million less, or total costs of $729.8 million.

A pessimistic, high cost estimate is obtained from assuming higher construction costs for the FFTF and GCFRE, and an even longer period of development. The FFTF might cost $100 million and the GCFRE $40 million, or the two together could cost an additional $22.5 million beyond the amount expected from "middling" construction results. The program might take 4 years longer than expected -- and any delay longer than that would be indicative of

Table 11.

Cost Components of Fuel Development for the Gas Cooled Fast Reactor

A. FFTF construction costs are $87.5 million.

FFTF design costs are $10 million.

FFTF operation expenses:

1967	1968	1969	1970	1971	1972	1973-1980	Total
7.2	9.0	10.0	10.5	10.5	10.5	15.0	$217.7 million

B. Fuel Materials Development:

1967	1968	1969	1970	1971	1972	1973-1980	1981-1984	Total
25.4	14.2	16.4	16.4	16.4	16.4	15.0	8.0	$257 million

C. The Gas Cooled Fast Reactor Experiment (GCFRE):

Construction costs of $30 million.

Design and operation expenses:

1969	1970	1971	1972	1973- 1894	Total
2.0	3.6	4.5	5.0	6.0	$87.1 million

D. Experimental Breeder Reactor-II

Operation expenses:

1966	1967	1968	1969	1970	1971	1972	1973-1980	Total
6.3	7.2	9.0	9.5	9.5	9.5	9.5	5.0	$100.5 million

Total for A, B, C, and D is $797.8 million.

a high probability of failure at an early stage, so as to require fundamental re-design of the entire fast reactor program. The four-year delay would add $96 million to costs. Total costs for development of a fuel element with these worst-likely results would then be $916.3 million.

Assume rather than the existence of an exclusive gas program, that there is also a project to develop a liquid metal fast breeder. The expenses of the two projects would then include some joint items; for one, an entirely separate FFTF would not be needed for gas research, but rather only special gas loops estimated to cost $15 million for construction and $3 million annually for operation. Fuel development expenses for gas alone would be about $5 million per year, an amount comparable to current spending on the carbide or alloy development programs. Since there would be fewer test positions available,[24] it would take several years longer to achieve the same results. Five additional years of FFTF operation would involve expenses of $3 million per year; GCFRE operating expenses would be $6 million per year, for each of these additional years as well, and fuel fabrication expenses would again be $5 million per year or $25 million more. New costs would be $70 million for gas as an incremental program, while old costs would be less by the construction costs of the Fast Flux Facility (and equal to $162 million). The GCFRE, testing gas exclusively, would have program costs of $117 million. Hence the total expected incremental expense would be $349 million. If success were achieved four years earlier, this expense would be reduced by $56 million which

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[24] Half the capacity in the FFTF is to be for fuel pin development and testing rather than for complete element and fuel subassembly testing. If the total number of gas test positions is reduced from six to four to accommodate two programs, then 50 per cent more time or an additional five years of irradiation is required to develop the fuel.

would make the total incremental cost $293 million. If, on the other hand, it took four years longer and the GCFRE cost $10 million more to construct, $66 million would be added to the costs, resulting in total incremental expenses of $415 million.

The Gas-Cooled Fast Breeder Reactor: Development of Core Performance

The core designs for gas-cooled commercial fast reactors cannot be undertaken before three research projects show at least preliminary results -- the same four projects taken on for the same reasons as in sodium and steam cooled fast reactor development. First, data have to be collected establishing the frequency distribution of neutron energy in a fast system with helium as the coolant; this can be used to plan the size and shape of the critical mass, and is followed by experiments on mock-ups of the mass. The third step is to build full-sized cores of different designs in experimental reactors to test performance under both normal and induced disaster conditions. Then enough results should be accumulated to evaluate alternative core configurations, to choose one of these, and to predict the behavior of the chosen configuration.

Most parts of this plan are the same as those in the development programs of other fast reactors. Data collection and the subsequent construction of mock-ups are done in the same manner and at the same cost as in the steam breeder program, even though the coolant is not the same. The project to test critical cores may differ, in content -- corrosion tests of the assembly in a steam or sodium environment may not be duplicated for the gas-cooled cores, because corrosion is not a problem of the same magnitude -- but the capital and personnel required to carry out the whole testing program are of

the same order of magnitude. Costs for fuel core development in a gas fast reactor are estimated to be the same as in the steam cooled reactor: at least $176 million, with middling research results $241 million, at worst $331 million. In the case of a gas cooled program incremental to a liquid metal fast reactor development program, the costs range from $148 million to $252 million (with middling results at $178 million, as estimated for steam cooled core development).

The Gas Cooled Breeder Reactor: Components and Systems Development

The extensive program for developing components for high temperature advanced converters is about to bear fruit, by installation and (hopefully) successful operation of pumps, heat exchangers, gas piping in the Peach Bottom and Fort St. Vrain reactors. Given good results then components development expenditures can be considered "incremental" and at the level proposed by industry:

Item	Cost
Gas Heat Exchanger	$1-2M
Gas Circulator	$1-2M
PCRV - R&D	$3-5M
-- Full Scale Mockup	$1M
	$6-10M

If $5 million is required to upgrade all other components to a level of quality slightly higher than for the advanced converter, maximum expected costs are only $15 million; if these estimates, formed by optimism, turn out to be off by a factor of one half, total costs would only be $30 million.

It is assumed, from a review of the standards for improvement and the present status of these components, that at most a contingency of 50 per cent is needed for the major components, so that present estimates of costs can be confined to \$15 million. In addition, control rod drives, valves, tubing, etc. will cost in the neighborhood of \$5 million more to develop. Hence total components development should lead to about \$20 million of expenditures.

In the event of extreme difficulty, costs would be as much as twice this projected amount. Given rather straightforward development problems, this is difficult to achieve. Similarly, if all goes extremely well, costs might be as little as \$6 million for major components and \$4 million for minor components, or \$10 million in total.

Even though gas coolant technology is in the process of being demonstrated in the United States in thermal systems with high efficiency, no fast gas system is now in operation or even at the prototype stage. As a result, systems development will be a longer and more expensive project than for other coolant programs. The gas prototype needs compare more with those for the BWR than PWR in the late 1950's: there will be a need for a number of prototypes in order for the industry to learn enough by doing to establish an efficient production base. The Boiling Water Reactor program had six reactors under 100 MW_w, and only one in the 300 MW_e range en route to competitive status. Thermal gas reactors contemplate a scaling up from Peach Bottom (35 MW_e) to 300 MW_e at Fort St. Vrain; this size increase will yield much information on whether further increases are feasible for the gas cooled fast reactor. But at least one prototype plant in the 50-100 MW_e range specific to the fast system is required to show technical reliability, and approximately three

plants in the 300 MW_e range are needed to proof test the second round of improved components. Then a 600 MW_e size plant would test out the final round of full scale components and core assembly for a 1000 MW_e demonstration plant. This is to reproduce, and expand, the present gas advanced converter program: one Peach Bottom size prototype will be essential to learn the technology of the system and three prototypes of the scale of Fort St. Vrain are required to proof-test all the gas components; the full-scale prototype reproduces the last step in the gas advanced converter program (a step not yet begun). The costs of these plants and related research are shown in Table 11, based on the research portion of the first plant costing \$300/KW, of the three prototypes \$200/KW, of the full scale prototype \$160/KW and the demonstration plant \$65/KW.

These are costs for a substantial program that does not achieve a startling breakthrough at any stage. They can be much higher if presently unexpected technical difficulties are encountered in the program -- such difficulties as have been endemic in Peach Bottom (which had an initial estimate of \$700/KW and was constructed for approximately \$1,380/KW). Certainly actual costs of twice that estimated are not unknown, at least on the earliest prototypes; increased understanding of the magnitude of development problems at later stages usually reduces the deviation from estimate to less than 25 per cent for later prototypes (such as Haddam or San Onofre). Of course, such "improved" estimates have been made later on the basis of later information; but assuming the advanced gas converters are now providing the equivalent of "later information," a high estimate is obtained by applying

Table 12

Costs for Gas Fast Breeder Research Reactors

(millions of dollars)

Year	Prototype Construction	Prototype Design	General Systems Development
1971	15	10	29.5 (1968-1971)
1972			4.8
1973	60	15	5.0
1974			5.0
1975	60	10	5.0
1976			5.0
1977	60	5	5.0
1978			5.0
1979			5.0
1980	96	10	5.0
1981			
1982			
1983			
1984	65	10	
	356	60	74.3

a factor of 2 to the estimates in Table 12. for the 50 MW_e and first 300 MW_e plant, and a factor of 1.25 to all subsequent plants. These costs are $145M more for plant construction; in addition, there is likely to be more R&D expense involved with poor results, primarily in the early plants, adding $40.5M to prototype costs. Total plant costs could be as great as $675 million.

On the other hand, it is possible that costs may be significantly less than expected. Those immediately concerned with development of this reactor state that it is possible to go directly from the 50 MW_e experimental plant to a 1000 MW_e prototype plant, given that gas components exist from the HTGR program and that core performance is size independent (whereas the LMFBR must face increased positive reactivity from sodium void with increasing size). Then good research results early in the program would make it possible to build only a 50 MW_e and a 1000 MW_e plant, which would make costs of all plants equal to $80 million. There would be an accompanying reduction in prototype R&D of $40 million while general research would cost approximately the same amount per year for all years through that of committment to a 1000 MW_e prototype (which would be eight years sooner, so that $20 million would be saved). Consequently, total costs of only $154 million would be incurred for research reactors under the best of circumstances and results.

The total costs for all aspects of this gas development program are expected to come close to $1,600 million, if all specific projects are able to come in on time with middling success (as described in Table 13.). With splendid success, the time schedule could be shortened to have a demonstration 1000 MW_e gas fast reactor in operation in 1983 (as planned for the liquid metal reactor) after having spend only $1,100 million. But there are good

Table 1

DEVELOPMENT GOALS AND COSTS FOR THE GAS COOLED FAST REACTOR PROGRAM

OBJECTIVES	PRESENT POSITION	PROGRAM TO TERMINATION OF PROJECT	EQUIPMENT REQUIRED BUT NOT YET IN PLACE	RESEARCH COSTS (millions of dollars)			
				LOW COSTS	DESIGN COSTS	HIGH COSTS	TIME
(1) FUEL ROD PERFORMANCE: a) 100,000 MWD/T at 800-1200°F for coolant, producing [illegible] KW/FT.	a) No operating experience of significance -- Peach Bottom just begun.	a) Irradiation in fast reactors -- FFTF if possible and GCFRE if built, possibly Dounreay.	FFTF GCFRE (Gas Cooled Fast Reactor Experiment)	729.8 (293.0)	797.8 (349.0)	916.3 (415.0)	1984
FUEL CLAD PERFORMANCE b) Develop clad capable of high pressures (1000 p.s.i.) and temperatures (1300°F).	b) No relevant experience.	b) Irradiation testing of fuel rods with different cladding materials and surface roughening in gas environment in FFTF and GCFRE.					
(2) FUEL PROCESSING. Develop an economically attractive recovery method.	Aqueous, volatility and pyro-processing techniques being developed by AEC for other fast fuel elements.	Small scale to pilot plant experimentation with all processes.		30.0 (30.0)	30.0 (30.0)	30.0 (30.0)	
(3) SUBSYSTEM: CORE PERFORMANCE a) Critical mass b) Reactor geometry for maximum neutron economy and stable operation. c) Energy spectrum under wide variety of conditions. d) Impact of loss of coolant on safety. e) Meltdown characteristics.	a) No Pu-fueled fast assemblies current in operation in U.S. b) Uncertainty on magnitude of coolant coefficients. c) Reactivity coefficients poorly known in fast, gas systems. d) No information. e) No information.	a) Testing of critical assemblies, test mockup of 1,000 MW_e core with control configuration. b) Critical experiments to determine coolant coefficients. c) Measure reactivity coefficients in GCFRE, critical experiments; determine cross sections. d) Tests in STF, PBF type facilities. e) Theoretical investigation and FARET-STF type experiments.	a) ZPPR type assembly. b) ZPR type experiments. c) Cross section measuring devices (ORNL, ANL machines). d) Experiments in GCFRE.	176.0 (148.0)	241.0 (178.0)	331.0 (252.0)	1982
(4) SUBSYSTEM: COOLANT SYSTEM a) Components such as gas circulators, heat exchangers, valves, alternate cooling systems, prestressed concrete reactor vessels, etc.	a) Small scale components in use at Peach Bottom. 300 MW_e size components in design for PSCC reactor (thermal). PCRV prototype tested.	a) Components development program.		10.0 (10.0)	20.0 (20.0)	40.0 (40.0)	1979
(5) SYSTEM DEVELOPMENT Instrumentation and control of entire system. Operation of a prototype in accordance with design specifications.	Peach Bottom just beginning full power operation. No fast gas reactors in existence.	Build 3 prototypes in 300 MW_e range, one at 600 MW_e, and then a 1000 MW_e demonstration plant.	3 - Prototypes (300 MW_e) 1 - Prototype (600 MW_e) 1 - Demonstration Plant (100 MW_e)	154.0 (154.0)	490.3 (490.3)	675.5 (675.5)	1987
				1,099.8 (625.0)	1,578.1 (1,067.3)	1,992.8 (1,412.5)	

( ) assumes that the gas cooled program is incremental to a full-scale liquid metal fast breeder program.

reasons for being prepared for the final results in 1987 at costs as high as $2,000 million.

Perhaps some part of these costs could be spared by attaching a gas breeder program to the trailing edge of a sodium program. The costs of "adding on" are shown in parentheses in Table 13; they indicate that this breeder might be developed for little more than $625 million, or -- after research results have been disastrously poor -- as much as $1,412 million. These gains from consolidation are achieved by cutting into the number of groups working on a problem, and making the results of each serve more purposes by giving them away to different types of breeders. They will be looked at in more detail.

Combining Fast Breeder Development Programs

The above programs are all geared to development of a reactor type determined by the coolant -- liquid sodium (LMFBR), steam (SCBR), or gas (GCFR). Each program is subdivided into major problem areas to be dealt with before construction of any reactor plants. The problems are in fuel element, fuel processing, core performance, equipment, building, and system performance. The approach consists of spending money on research to solve the problems that stand in the way of putting together a reactor demonstrating that concept -- without regard for other concepts. Another, quite different approach is to conceive of the development of fast breeders as fundamental problem solving in fuel element and core performance regardless of the coolant, followed by quite specific improvement in the performance of subsystems built for particular types of coolants. For instance, the development of fuel materials -- be it oxide, carbide or alloy -- is a problem

common to all reactor types but the cladding must be developed independently for each type of coolant so that finding the proper clad material is a specific problem. This suggests that there are mainstream developments in control of fission behavior in uranium carbide vital to all fast breeder reactors and there are supplemental developments of metallic alloys for cladding required before an assembly of fuel rods is put together to run within the specific sodium environment or specific gas environment.

The research effort can be organized to allow the breakdown of costs between a "mainstream effort" and separate "supplemental efforts" for each concept. At the very least, such a breakdown shows a substantial degree of commonality in all of the separate programs discussed above, and also points out the costs of adopting parallel programs for two or more fast breeder types once a single large-scale program of basic fuels research is in operation.

Development of fuel elements is the most expensive part of any separate fast breeder program. New elements are required to sustain fast fission. Attaining fuel lifetime similar to that in thermal flux will be difficult: irradiation damage is more severe in fast fluxes because of deeper flux penetration of fuel rods and higher percentage burnup (required of fast fuels to compensate for higher-valued plutonium inventories). The total cost of development of economic fuel elements has been estimated to be the following for the three major concepts developed independently:

Concept	Costs of Fuel Development (millions of dollars)
LMFBR	645
SCBR	661
GCFR	798

If all of the above concepts were to be developed at the same time, the costs for fuel development would be less than the sum of these amounts; common expenses in fabrication and in testing particular alloys would not need to be duplicated. In particular, the neutron energy characteristics of fuel pin materials -- oxides, carbides, or alloys -- must be known to construct a fuel element regardless of coolant, so that the part of development related to fission material is common to all three programs. Then those costs of the FFTF and EBR-II irradiation facilities incurred for use in fuel materials testing, as distinct from fuel clad and subassembly testing, are common to all the programs and should be charged to "mainstream effort." In the present EBR-II test facility 44 per cent of the experimental space is devoted to structural or clad specimens, so that 66 per cent of total costs can be assigned to mainstream research. Later in the program, as knowledge of fuel material grows and the date for prototype construction approaches, clad and subassembly testing will become more important as the total fuel element must be tested and at least 60 per cent of the costs for tests will be for clads and subassemblies. Then roughly 50 per cent of the total irradiation program is aimed at developing economical fuel centers for general use in any of these programs. This fraction of effort is allotted to fuel in the metallurgical research done outside the large test facilities, as well. But the experimental plants designed to provide specific coolant environments are not divisible between general and supplemental research -- they are quite specific to developing reactor types. Given these rules-of-thumb for cost allocation, the expenses for fuel development are as follows:

Fuel Element Development Costs
(millions of dollars)

Item	General	LMFBR	SCBR	GCFR
1. FFTF	134	134	134	175
2. Fuel Metallurgy	133	132	132	128
3. EBR-II	55	55	36	55
4. Other test facilities	--	--	104	118
	322	321	406	476

Then the expectations are that specific costs of fast breeder fuel development are lower for the systems with the most experience -- the LMFBR and SCBR -- and highest for that with the least experience -- the GCFR.

Similar allocations can be made with the other parts of the separate programs. Fuel processing requires knowledge which is useful for any concept, so that the $30 million attributed for this to each program applies to all the programs and is added to the general fund. Core performance information is partly general and partly specific to each concept. Basic nuclear data, from cross section energy measurements, are one half specific to a coolant and one half general information. The integral and critical experiments are all related primarily to specific designs so that these expenses can be assigned to separate concepts. Theoretical studies, while partially modified by the type of coolant, seem applicable to all concepts and their costs can be assigned to the "mainstream effort," too. Finally, safety research centers on individual prototypes and costs from each can be assigned to specific concepts. Then the total expenses for core performance development are divided as follows:

Core Performance Development Costs
(millions of dollars)

Item	General	LMFBR	SCBR	GCFR
1. Reactor physics	145	100	100	120
2. Safety	--	135	135	135
	145	235	235	255

Component development projects are aimed at better equipment for control of the core or operation of the coolant system, centering on such items as control rod drives, heat exchangers, pumps, seals and valves. Practically all of the expenses for these are then related to individual types of reactors. Heat exchangers for sodium are different from those for gas and there are no such devices in steam fast breeder designs at all. There are exceptions -- for example, concrete pressure vessels are potentially useful in designs for all concepts and might be a general project, but even here it is central in the gas designs and peripheral in designs for the other two reactor types, so that its costs are attributed directly to the GCFR. The costs for all components specially constructed for use in reactor experiments, when allocated to specific concepts, come to $145 million for the LMFBR program, $10 million for the SCBR and $20 million for the gas cooled reactor programs. They show the manifold problems to be solved in a secure heat exchanger for sodium.

Systems development projects do not differ when more than one type of reactor is being constructed at a time from those for entirely separate programs. A reactor experiment with a sodium heat exchanger cannot be used to

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learn very much about energy production from a plant with a direct steam circuit. Development expenses are unique to each concept; they are (as shown from Tables 7, 10, and 13) $300 million for the LMFBR, $150 million for the more modest experiments with steam cooled systems, and $490 million for the less-developed, gas-cooled reactor designs.

A broadly-based research project to build an efficient fast breeder, whatever the coolant concept, would build from the general or "mainstream effort" to one or more specific program with one or more coolant concepts. The cost of the basic work in fuels is estimated to be $467 million, given modest targets and a middling success in achieving these targets. The added costs of their taking on reactor types are $1,001 million for the LMFBR, $801 million for the SCBR, and $1,241 million for the GCFR. Such costs would occur from setting strategy for breeder research as in the thermal reactor program -- the strategy being to take on all of these concepts until one of them showed results that justified given exclusive attention to that concept.

Consider such a strategy. If the cut-off of all except one took place very early in the second stage (at the mid-point of fuel performance development, say) then the costs for one successful reactor may be little different from the expected costs of simply starting out with only that reactor. At least the total expected costs from an early choice of the LMFBR are $467 million + $1,001 million under the combined strategy, as compared to $1,591 million from always concentrating on this reactor exclusively (as shown in Table 7). The costs of the expanded general research in fuels weighs more heavily on the steam and gas programs, because of the need for so much specific

development in these cases. But not much more: the sum of general and specific research is only $100 million to $200 million more than the costs of exclusive programs (shown in Tables 10 and 13) in steam or gas-cooled reactors.

There are manifold possibilities for results from combinations of projects, and for expenditures to produce these results. The most interesting are suggested by the combinations promising the most in gains for society, at the least research risk and cost. They are dealt with in the next chapters.